THE

ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

VOL. II. JULY, 1812. No. VIII.

SELECTED PAPERS.

On the non-existence of Sugar in the Blood of persons labouring under Diabetes Mellitus. In a Letter to Alexander Marcet, *M. D. F. R. S. from* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S.*

[From the Philosophical Magazine for July 1811.]

MY DEAR SIR,

IN reply to your inquiry respecting my experiments upon the non-existence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the Diabetes, (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it;

for as I soon after* relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits which are generally interesting to the Royal Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments, requesting that you will in return state, more fully than you have hitherto done, the result of that further step in the inquiry which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well-known talents of Mr. Cruickshank, it appears that they "had not been so fortunate as to obtain a sufficient quantity of serum for chemical experiment†;" and were unable fully to satisfy themselves, by the taste or by other means which they could employ, concerning the existence or non-existence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

For the purpose of forming some judgment on this question, Mr. Cruickshank made trial of the quantities of oxalic acid that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

This method, it is evident, is liable to a twofold objection: first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

* In 1800. † Rollo on Diabetes, p. 408.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation.* To six drachms of serum I added half a drachm of muriatic acid previously diluted with one drachm and a half of water, and immersed the phial containing them in boiling water during four minutes The coagulation was thus rendered complete. In the course of a few hours a drachm or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness that ap-

* I presumed that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralizing it.

pears after evaporation; occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness and disposition to deliquesce are of course more manifest than with smaller quantities.

As I was aware that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

As a further test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and if further heat be applied, it becomes black in proportion to the quantity of sugar present.

Such are the appearances when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction at the time when these experiments were conducted, and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself

as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

The first which I examined was a portion of blood that had been taken from a person whose urine had been analyzed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

In a third instance, I had some serum from the blood of a person whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis.) After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic aid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

I had also a fourth opportunity of examining serum of a person whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty-six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat, as

sugar is: and its refractive power* was too great for that of sugar.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

In the next experiment I added half a drachm of the urine of the same person to six drachms of the serum, and with a due proportion of diluted muriatic acid coagulated as before. Although the quantity of extract added did not exceed $\frac{36}{160}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

The result of these trials was such as to satisfy me that the serum in this instance contained no perceptible quantity of sugar, or at least that the water separable from the coagulated serum did not contain one-thirtieth part of that proportion which I had found in the urine of the same person.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood-vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it

* The method by which this was tried has since that time been described in the Philosophical transactions for 1802.

* Account of the retrograde Motion of the absorbent Vessels, by Charles Darwin.

was conveyed to be the absorbent system, upon the supposition that they might admit of a retrograde motion of their contents.

Without adopting the theory of Dr. Darwin, it did appear to me that the fact deserved to be ascertained by some test more decisive than nitre; and I conceived that if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

After a few previous trials of the period when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person about thirty-four years of age was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect; and as I thought that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern that any degree of blueness was occasioned by it.

This experiment having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

Being desirous of ascertaining whether the prussiate could be discovered in any other secretions, I have repeatedly exa-

mined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

I have also, during a severe cold, accompanied with profuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

It was nearly in this state that I left the inquiry at the period I have mentioned, and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance by which substances may reach the bladder.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope that we might from that source derive some explanation of such phenomena. But though I have referred* secretion in general to the agency of the electric power with which the nerves appear to be indued, and am thereby reconciled to the secretion of acid urine, from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances may equally be effected by the same power as acting cause, still the channel through which they are conveyed remains to be discovered by direct experiment.

I have, indeed, conjectured that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu;* but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject. I remain, dear sir, with great regard, your's very truly,

W. H. Wollaston.

* Phil. Mag. for June 1809.

History of fatal Effects from the accidental Use of White Lead; in a Letter to the President. By JOHN DEERING, *Surgeon, F. M. S.; with additional remarks by* WILLIAM SHEARMAN, *M. D. F. M. S.**

[From the Philosophical Magazine for August 1811.]

AT the sitting of January 30, 1809, a verbal communication was made to the Society, by the author of the following memoir, of some extraordinary symptoms, followed by the death of several individuals of a family whom he had attended. It appearing to the members present highly probable that these unfortunate events originated from the poison of lead†, a committee was deputed to investigate and to endeavour to detect the real cause of the fatality; which the following relation fully and satisfactorily explains.

Aldersgate Street, Oct. 4, 1809.

If the following narrative do not convey any important medical information, it may not be wholly uninstructive, as it relates to a domestic calamity, occasioned by a circumstance which at the time was wholly unsuspected; and it may at least inculcate the necessity of a closer investigation of symptoms from causes not fully ascertained, and at the same time evince the fallacy of hasty prognostics.

On the 21st of October last, I was desired to visit Mrs. R., the wife of a respectable tradesman in Aldersgate-street, who complained of violent pain in the scrobiculus cordis, with great soreness of the epigastric region when pressed upon. She had vomited a considerable quantity of bilious matter, and at the same time her bowels were constipated: the pulse was calm and regular, the tongue clean and moist, and there was no symptom of fever present. She immediately took a cathartic,

* From the Transactions of the Medical Society of London, vol. i. part i.

† On the following evening Dr. Shearman delivered the annexed communication, which served to confirm the probability of these suspicions; although Mr. D. had been hitherto unsuccessful in detecting the precise origin of the exciting cause.

which operated, and an opiate in the evening. The following morning the patient appeared relieved; in the evening, however, the pains and vomiting recurred, and these symptoms continued for some successive days, in so distressing a degree, that it was deemed advisable to consult the family physician, which was done on November 4, 1808. At this time these symptoms continued as already intimated, without any appearance of fever, and hence the physician was induced to consider the affections as of a rheumatic and spasmodic nature.

In a few days, in consequence of the amendment of the patient, he discontinued his visits. In about a week after this period, a boy in the same family, nearly sixteen years of age, was seized with symptoms exactly similar to those of the preceding case, and similar remedies afforded only partial relief, till at length he was removed into the country, and thereby recovered his health.

A week after the attack of this youth, the eldest child, a boy six years old, was also seized with analogous symptoms, and, the mother having relapsed into her former state, the physician was again consulted on the 19th of November. At this time three other persons in the family laboured under similar affections, and suspicions were now entertained that some poisonous substance might have caused this general indisposition of the family; but after minute investigation, no one circumstance was discovered to confirm this suspicion, or to elucidate the source of so extensive a calamity.

The sickness and pain continued unabated in Mrs. R.; but the son, after the period of a fortnight, was deemed in a state of convalescence by his physician, who discontinued his attendance; he was, however, soon after seized with convulsions, and expired within a few hours. Unexpected and severe as this shock was, Mrs. R. afterwards gradually grew a little better. She had hitherto continued to suckle her child, which, it being fifteen months old, she was advised to wean: to this she reluctantly consented. In about ten days afterwards the child became somewhat costive, without any other apparent indisposition; but at this period it was seized with vomiting and convulsions, and suddenly expired. The unhappy parent now experienced a return of her complaints, and, under a persuasion of the

inefficacy of professional aid, she was prevailed upon to consult an empiric, whose attendance, though continued to the end of the year, proved unavailing; and on the 3d of January, 1809, she had the advice of Mr. Chevalier, an experienced surgeon, who considered the patient's complaint to be chronic rheumatism; and by the use of clysters of warm water, oily mucilaginous medicines, fomentations, and vesicatories, she appeared to experience more relief than at any period since the first attack; but, although the vomiting and sickness were less violent and frequent, the pain and soreness of the abdomen, first complained of, never entirely subsided: she was, however, able to sit up and amuse herself with a little needlework, and even to go about the domestic concerns of the family, and Mr. Chevalier had proposed to pay his final visit on the 21st. On the morning of this day she rose at ten o'clock, and within the space of an hour afterwards, whilst standing near the desk of drawers, she suddenly exclaimed, " I am dying!" She was seized with convulsions, which continued till five o'clock in the afternoon, when she expired.

On the subsequent day, Mr. Chevalier, whose anatomical skill is well known, examined the body by dissection. Neither the thoracic and abdominal viscera, nor the brain, upon the most minute examination, exhibited the least appearance of disease; in short, not the least trace could be discovered of any morbid affection.

With respect to the three other persons already mentioned to have been indisposed, the servant maid, one of them, was conveyed to her friends, and recovered. A sister-in-law of Mrs. R. also recovered; but the third, who was her mother-in-law, died, after lingering under disease till March.

These circumstances having been cursorily communicated to the Medical Society, Dr. Adams, Dr. Hamilton, and Mr. Lawrence, were requested to visit the house of this unfortunate family, and to endeavour to ascertain the cause of the calamity. Every culinary article and the whole premises were accurately examined, but without its leading to any discovery. It appeared, indeed, that Mr. R., the husband of the deceased lady, had purchased a cask of sugar at a sale, a considerable part of which had been disposed of to some friends in the coun-

try, who had used it without inconvenience, and hence no suspicion was entertained of this article having produced the fatality in Mr. R.'s family.

In this state of uncertainty, Dr. Laird, another member of the Medical Society, visited the house; and, on examining the cask which had contained the sugar, he observed a white powder adhering to its inner surface, and which, on being heated by the blow-pipe on charcoal, afforded globules of lead in the metallic state.

The mystery was thus at length developed. The sugar had been injudiciously put into a cask which had previously contained white lead. That part of the sugar which was sent into the country had probably been taken out of the middle of the cask, and had never come in contact which the lead; whilst that which was used by the family, having been taken from the side, was impregnated with this metal, and doubtless was the source of the fatal events described.

Of nine persons in this family, who were more or less indisposed, four died, and the effects of the poison appear to have been nearly in the ratio of their respective ages.

The infant, fifteen months old, was attacked and expired within the space of twenty-four hours; the child six years of age survived a fortnight; Mrs. R., aged forty, lingered three months before the fatal event took place; and the mother-in-law, aged sixty-seven, died four months after the attack.

The symptoms in each were very similar. The vomiting, pain in the stomach, and costiveness, marked the attack of the disease; and the soreness of the epigastric region in those who recovered was not removed by medicine, but seemed rather gradually to wear away by time or change of air. The matter vomited was usually of a dark yellow colour, though sometimes green; the fæces were in general dark-coloured; but in the case of Mrs. R. they were completely white during the space of twenty-four hours only.

There was a considerable sameness in the medical treatment. The opiates which were given afforded no mitigation of the symptoms, unless joined with cathartics, and aided by fomentations, &c. The countenances of all the patients exhibited a

pale, sickly, wan aspect. The pulse in each was slow and regular, rather indeed sluggish, and generally below the natural state; but in no instance was there any symptom of paralysis.

J. DEERING.

Experiments upon Cantharides.

By M. ROBIQUET, Apothecary.

[From the New Medical and Physical Journal, November, 1811.*]

AMONG the number of experiments made upon cantharides, those of Drs. Thouvenel and Beaupoil are principally distinguished; but after bestowing upon each of these physicians all the merit due to them, for having published some interesting results on the chemical nature and remarkable properties of these insects, it must be allowed we are far from possessing an accurate analysis of them, and consequently that it is impossible, at present, to explain in a satisfactory manner, the various effects produced on the animal economy by cantharides.

Fully persuaded of the advantages which would accrue from a more minute investigation of their chemical analysis, I undertook a series of experiments, the results of which I shall presently relate; but I wish previously to state what our present knowledge upon the subject is, as derived from the abovementioned works of M. M. Thouvenel and Beaupoil. The former gives as the products of his analysis,

1. Parenchymatous matter, forming one half the weight of the cantharides.

2. A black matter, affording by distillation an acid phlegm, and a concrete volatile salt.

3. A greenish greasy matter of an acrid taste, to which he attributes the whole of the odour and causticity of the cantharides.

4. Lastly, A yellow substance, which he regards as wax, not fully elaborated, meriting no consideration in a medical point of view.

M. Beaupoil, in the recapitulation of his chemical analysis, says, that cantharides contain,

* (Annales de Chimie, 1811.)

1. A black extractive matter soluble in water.

2. A yellow matter, equally soluble in water, and separable from the former by alcohol.

3. An unknown acid, but suspected to be the phosphoric.

4. Lastly, A parenchymatous substance insoluble in these fluids, and composed for the most part of an animal matter and phosphate of lime; sulphate, muriate, and carbonate of lime and iron being in very small quantities.

The same author is induced to believe from his physiological experiments, that cantharides contain two principles possessing similar properties; one the green matter, whose action is limited to simply vesicating the skin when applied to it, producing no effect upon the general animal economy; the other the extractive matter, which has the double property of vesicating the skin when applied to it, and is also highly deleterious when introduced into the digestive or circulatory system. This extractive matter is said to be composed of two substances, the one yellow and the other black, to both of which M. Beaupoil attributes a powerful vesicating quality.

It is very difficult to suppose that three different substances should each at the same time possess a quality which is so very rare; and I suspected that this effect, which is so remarkable in cantharides, must be owing to one and the same matter only; upon this supposition, I made a great number of trials, the most conclusive of which I shall detail.

I boiled some cantharides slightly bruised in distilled water; the decoction was of a brownish red colour; it reddened turnsole, and possessed also some other properties which I shall pass over in silence, as being irrelevant to the present subject of inquiry, whose sole object is to investigate the nature of the vesicating matter. I shall, therefore, content myself with saying the decoction possessed this property in a very high degree. Finding that water took up this vesicating substance, whatever it was, I continued boiling the same cantharides until the water was no longer charged with any principle; I then dried the residue and treated it with alcohol, and the green tincture produced from it was freely exposed to the air, to obtain by slow evaporation whatever was contained in solution; by these means I collected a green fluid oil which was not in

the least vesicating. I applied some of it to my lips without their being the least affected by it. Convinced by this, that we had erróneously regarded this green oil as one of the vesicating principles, I examined the aqueous decoction, in order to discover the source of its vesicating property.

I reduced the decoction to the consistence of a soft extract; and in imitation of M. Beaupoil, I separated this extract by means of alcohol, into two distinct and separate parts, one black and insoluble, the other yellow, viscid, and very soluble. Perceiving that the portion soluble in alcohol was strongly vesicating, I was not at all afraid of taking up the black matter, even by the repeated action of boiling alcohol, till it was no longer coloured, notwithstanding M. Beaupoil's advice. I took care to stir up well, with a glass rod, the residue at the bottom of the hot alcohol, so that the whole surface might be successfully presented to it. Having done this, I treated the black matter with water, and was satisfied that it had not preserved the least vesicating quality. Here then, were two essential points determined.

Having thus succeeded in tracing the vesicating principle to that portion of the watery extract soluble in alcohol, nothing more was wanting than to know if the whole of this matter, or a part only, was vesicating. It was a long time before I could come to any conclusion on this subject; I did not fail, however, to submit this substance to a variety of trials, but all the products which I obtained from it were equally vesicating. At last, I treated this yellow matter with rectified sulphuric ether, which at first appeared to have no action upon it; but after having introduced some of the mixture into a vessel hermetically sealed, and agitated it almost continually for several hours, this substance, which in the first instance remained fixed to the sides of the vessel, detached itself by degrees, and was not long in becoming soft and separating. The ether took a slight yellow tint; I decanted it into a porcelain saucer, and as the ether evaporated, small micaceous plates, stained by little drops of a yellow fluid, were deposited. When the evaporation was completed, I treated the residue with cold alcohol, which took up the yellow matter without sensibly attacking the little crystalline plates; these were col-

lected upon bibulous paper and dried; they were insoluble in water, soluble in boiling alcohol, from which they separated upon its cooling, always taking a crystalline form. I employed the above method to obtain this matter in a state of purity, and when I judged it to be so, my first care was to satisfy myself if it was vesicating. For this purpose, I fixed about the hundredth part of a grain at the extremity of a slip of paper, which I applied to the edge of my under lip; in about a quarter of an hour, I began to feel a slight pain in passing my finger over the place; soon after some little blisters formed. Once certain of what I was inquiring after, I put a small quantity of cerate upon the part, to arrest, or at least diminish the effects of the vesicating substance; but it happened that the cerate, by diluting the small quantity I employed, extended its effects over a very considerable surface, and I had both lips covered through their whole extent with bladders filled with serum. I took a very small quantity of this substance and diluted it with two or three drops of oil of sweet almonds; the solution being complete, I covered a small square of varnished paper with this oil and applied it upon my arm; in about six hours a bladder was formed, the whole dimensions of the paper, leaving no doubt that this matter was essentially epispastic. When on the other hand, I examined in a similar way, the yellow matter from which the ether had extracted the vesicating principle, (for so in future I shall call it) I could not discover in it the least trace of this property.

Nothing is more easy than to conceive how this result, notwithstanding its simplicity, had escaped M. Beaupoil. The cause must necessarily be in the manner of operating; indeed, this physician had only infused the cantharides in hot water; but as the vesicating principle is not soluble by itself in water, as I have already shown, it could only be continued in it by means of some other substance, such as the yellow matter; it could, therefore, only be totally taken up by this vehicle by increasing its power to the utmost, especially if we consider that the green oil remaining in the cantharides after the watery infusion must retain, as an oily body, a great affinity for the principle in question, since it is soluble in oils. Besides, M. Beaupoil directs that the watery extract should be treated by

alcohol in the cold; for, says he, if the slightest degree of heat be employed, the substances which have been separated will again unite. For my part, I have not observed this to be the case; it is true, that before putting the extract into alcohol, I dried it as much as possible without injuring it, and employed highly rectified alcohol; I also used a large quantity, otherwise the humidity which remains in the extract, dilutes the alcohol, and favours the solution of the black matter, especially if heat be employed; but in following the mode I have pointed out, the black matter is not attacked even by the boiling alcohol; on the contrary, it becomes more dry and friable in proportion as the yellow matter is taken up, even so as to be no longer softened by the heat.

I think I have sufficiently shown, not only that the green oil and the black matter are not at all vesicating, as has hitherto been thought, but also that this yellow substance characterized by its solubility in alcohol and in water, owes this singular property to a particular matter which may be separated from it by well rectified ether; that this matter, insoluble in water, is soluble in boiling alcohol; that it is deposited by cooling, in crystalline plates similar to spermaceti; that it is soluble in any proportion in oils; and this last property agrees well with the daily practice of pharmaceutists in forming their epispastic preparations.

After having resolved the first question proposed, I endeavoured to ascertain the nature of this hitherto unknown acid; for this purpose, I undertook a new set of experiments, which I shall now proceed to relate.

I commenced by putting cantharides to infuse in cold distilled water; twelve hours afterwards I filtered this solution; it was of a deep brown red colour, reddened turnsole, and coagulated by heat, the coagulum resembling the ordinary scum of meat. Being again filtered, the infusion, always acid, afforded by means of lime water, an abundant floccy precipitate. The oxalate of potass had nearly the same effect; with acetate of lead the precipitate was very considerable.

Of all the re-agents I employed, ammonia the most engaged my attention, because it led to the most satisfactory results. I have already said, that the infusion of cantharides was acid,

and it happens that when this acid is saturated by volatile alkali, there is instantly formed a crystalline precipitate rather yellowish; after the saturated infusion has stood quiet for a few hours, there is no longer any precipitate formed by lime water, as took place before. We must conclude from this experiment, that the phosphoric acid, whose presence in the infusion was indicated by the lime water, makes a part of the precipitate obtained by the saturation, because this acid is no longer to be found in the liquor; and as, on the other hand, the ammonia takes up only the uncombined acid, it necessarily follows this cannot be the phosphoric acid, as M. Beaupoil had conjectured.

This crystalline precipitate, when rubbed with caustic potass, gave out a great deal of ammonia; it dissolved very readily in distilled vinegar, and upon adding to the solution a few drops of acetate of lead, a white precipitate was formed; upon cooling, it took the polyhedron form, which characterizes the phosphate of this metal. Hence it appears, that a triple phosphate is precipitated from this infusion by means of the volatile alkali. In order to determine the other base of this triple phosphate, I took a certain quantity of it, and calcined it, to drive off the ammonia; I then treated the residue with sulphuric acid; the solution, which was perfect, except a few floccules, being evaporated, there was left a gelatinous mass, as if it contained a great deal of silex. Thinking that the excess of sulphuric and phosphoric acids which was contained in it, might, by their mutual affinity for the same base, prevent the crystallization, I took up this excess by alcohol, and the residue treated by water, gave a prismatic salt of a bitter taste, having all the properties of sulphate of magnesia. Thus it may be regarded as fully demonstrated, that the infusion of cantharides contains phosphate of magnesia, which is held in solution by an uncombined acid, different from phosphoric, and that this phosphate is precipitated in the state of a triple salt by adding ammonia. This result, true as it is, will appear the more singular, as M. Beaupoil in his analysis of cantharides, makes no mention of this salt, although it is found in them in great quantities.

It is possible that this magnesian salt formed a part of the

bony frame of the cantharides; but if their bones have so little density, as to permit the phosphate of magnesia to be taken up by the acid even in the cold, how does it happen, that the phosphate of lime, which is the basis of them, and which is so abundant in the remains of cantharides, when treated by water and alcohol, is not equally dissolved, although it is not quite so readily attacked?

It remains now to ascertain the nature of the acid contained in the infusion of cantharides, but it may be easily conceived, how difficult it is to separate it from all the substances with which it is mixed. I have often thought I had separated it by means of different re-agents, but on examining the products attentively, I perceived my error; at last, after many ineffectual trials, I succeeded in obtaining it by a very simple method.

It is sufficient indeed to infuse bruised cantharides in ether at 60° of heat; this becomes of a dirty yellow colour after two or three days' maceration. This ethereal tincture is to be decantered and poured into a shallow capsule; the ether in evaporating leaves a reddish yellow oil, and when it is entirely dissipated, there remains a little colourless fluid separated from the oily matter. This liquor strongly reddens turnsole, is not precipitated by any re-agent, and affords by distillation an acid product possessing the same properties; by these characters, I recognised the presence of vinegar. It is almost needless to say, the same ether afforded no acid by its evaporation, when it had not been macerated upon cantharides.

Fearing lest the acetic acid, obtained in the preceding experiments, should proceed from the vinegar commonly made use of to kill the insects, I took some live cantharides, which had never been submitted to the action of any foreign substance. After bruising them in a mortar, I immediately introduced them into a glass retort, and observing every requisite precaution, I distilled them in a salt-water bath. The product condensed in the receiver was a little milky, and strongly impregnated with the odour of cantharides, without any thing penetrating; it reddened turnsole paper, but this did not take place very speedily. The earthy and metallic salts produced no precipitate in this liquid. We find, therefore, that this product contains acetic acid, but in very small proportion, and,

moreover, that the infusions or decoctions of the recent cantharides afford as unequivocal signs of acidity as those of commerce. Hence arise new doubts, which require new experiments; I therefore took some of these fresh cantharides, and boiled them in distilled water; the filtered decoction was evaporated, and during its evaporation I remarked, that a sediment was thrown down of an earthy appearance, and which was much more abundant than what took place under similar circumstances from cantharides, which had been collected some time before. Before the decoction attained the consistence of thin syrup I stopped the evaporation, to separate the sediment, which I washed in cold water, and found to possess the following properties.

This sediment commonly appears in the form of a grained powder of a yellowish-gray colour; applied with a little water to turnsole paper, it reddened it; it grates under the teeth, and has a little taste; thrown upon burning coals, it diffuses an odour of animal matter; it is partly dissolved in boiling distilled water, from which it separates on cooling, in the form of gray floccules. The solution, when poured off from these floccules, affords a precipitate by the acids, and by nut-galls; alkalies, and especially lime-water, also produce a precipitate from it; some of the sediment in question being rubbed with caustic potass, it formed a soapy paste, which dissolved for the most part in water. The alkaline solution, being filtered, was abundantly precipitated by acids; and when the solution is in a very diluted state, and a considerable excess of acid is poured into it, the precipitate, which slowly takes place, affects a crystalline form. These small crystals dissolve in concentrated nitric acid with a great effervescence and separation of nitrous gas. The solution, evaporated to dryness, gives the fine rose colour which characterizes the uric acid. I am equally sure, that besides the uric acid, this sediment contains a little phosphate of magnesia, and a certain quantity of animal matter.

Although the presence of uric acid in cantharides has nothing surprising in it, yet it is, however, very curious that these insects, which produce so remarkable an action on the urinary organs, present in their composition so many points of analogy with the urine itself.

Before I conclude this article, I must remark, that cantharides long kept, do not furnish the same products. I could never discover any uric acid in them; and had I not many times obtained this acid from recent cantharides, I should have considered this result as being involved in some doubt.

I said, in relating one of the above experiments, that ether macerated upon cantharides, afforded, besides the acid I have mentioned, a reddish-yellow oily liquid. I treated this fluid in the cold with rectified alcohol, but as no effect was produced, I employed heat. This oily matter fell to the bottom, and united into globules without being dissolved; the alcohol was, however, slightly coloured. I poured it off, and upon cooling, it deposited a great quantity of micaceous plates, which proved to be the vesicating principle; it was united to a very small portion of greasy matter, from which it was disengaged by dissolving it several times in boiling alcohol.

As to the other substance, it is really a fatty matter, which has all the properties of the fixed oils; it is distinguished from the green oil, which I have before spoken of, by its insolubility in alcohol; but neither of them is at all epispastic. This is then to be regarded as an additional product in the analysis of cantharides.

I was much surprised by obtaining a yellow oil when ether was employed, which was never obtained by any other method; some experiments, however, have led me to believe, that this oil, although insoluble in alcohol, forms a part of the yellow substance; this fact, though it appears extraordinary, is only a confirmation of what we frequently find takes place in other cases.

As I have thought it of most importance to insist particularly upon some material circumstances, the better to understand the particular object of my present inquiry, I have not observed the order requisite for a methodical analysis; intending, in the second part of this Memoir, to describe the progress and detail of my experiments; which second part will also contain a particular analysis of each individual substance I have announced in this.

The great advantage which will result from this essay, is undoubtedly the being able, correctly, to appreciate the medical

virtues of each of the products of cantharides; to determine exactly to which of them any particular effect is owing; to discover which may assist the powers of the others in any circumstances; and lastly, what substances may increase or destroy their activity. These results, so highly important to the healing art, can only be obtained by a great number of experiments, requiring besides such an extensive knowledge, that I cannot flatter myself with being able to conduct them successfully. I therefore hope, they will so far interest some other person, as to induce him to undertake those parts which more particularly relate to medicine; and I have then no doubt, but these experiments will altogether form a very interesting work upon the subject.

On the Treatment of Gout.

By Mr. WANT.

[From the Medical and Physical Journal for September, 1811.]

Ὑπολημψιν δε εκ τυτυ το παθος ανιαραν εκτησατο ϗ μηδὲ ὅλως ὑπο τεχνης ιατρικῆς ἰαθῆναι ποτε δυνηθῆναι· ἐγω δὲ φημι ὡς εἰ διαγνωσθεῖεν καλως αἵ τε διαφοραι ϗ τα ειδη αυτης ὅσα τὲ ϗ οἷα τυγχάνη, ευθεραπευτος ἂν βαδιως ὑπο των ιατρων γενησεται.

"An opinion has been entertained that this disease is not curable by the medical art, but, I affirm, that if physicians make a proper distinction between the different species of the disease, it may *very easily be cured.*"—ALEX. TRALLIAN, περι ποδαγρας, lib. xi.

"Est enim remedium hoc tantæ efficaciæ, et hominem totum liberum a Podagra, et articulario morbo præservet, ita ut eo qui utitur, nihil ei opus sit de sanguine extrahendo aut remediis aliis, ut *longa experientia compertum habetur.*" LUDOVICUS ENRIQUES, De Podraga.

No subject of late has more engrossed the attention of the profession, and, perhaps, of the public at large, than the nature of gout, and the power of the French medicine in alleviating the paroxysms of that distressing malady. That a disease generally considered incurable should now be relieved, as it were, by a charm, could not fail to excite the wonder, and rouse the energies of the medical world. The effects of this medicine were no sooner acknowledged, than experiments were instituted in all quarters, as might naturally be expected,

with the view of discovering its composition; but a period of nearly eighteen months had elapsed without any communication being made to the public upon the subject, until the appearance of Mr. Moore's Tract, in which he endeavours to prove its identity with a mixture of hellebore and opium. The reasonings in support of his opinion, as far as a cursory view of his paper has enabled me to judge of them, are conducted with much ingenuity; but whether he will succeed in establishing this point I shall not take upon me to hazard a conjecture. I must confess, indeed, that I attach less importance to this part of the inquiry than many may be disposed to give it. When first the medicine was administered in this country, when its effects were in a measure veiled in mystery, when more was attributed to it than subsequent experience proved to be warranted by fact, when it was supposed to possess properties totally unlike any other remedy in use, the inquiry was most natural. In the course of time, however, a long succession of facts has taught us so much of its operation as to supersede, in my opinion, the necessity of any new attempt to imitate it. Daily experience shows us, that a gouty paroxysm is curable by the production of certain sensible effects on the constitution. If any one medicine, therefore, or combination of medicines be discovered, which is capable of producing those effects, it is a fair inference that the gout may be cured with as much certainty in the one case as in the other, and the scientific physician will have no cause to regret his inability to compound a medicine, whose colour, taste, and smell, are similar to that which led him to the discovery. Under these circumstances it is only as a matter of curious speculation, that I should consider the farther prosecution of the inquiry as deserving attention.

Should Mr. Moore's opinion of this medicine, and the source from which it is derived, be supported by additional evidence, the profession will learn to set a higher value on the labours of antiquity than they have hitherto done. Their valuable repositories, in general, record the language of experience; and though much which they contain has been rendered useless by the efforts of succeeding generations, there yet remains a copious harvest for the industrious investigator.

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From all that we can collect respecting the sensible operation of the gout medicine, it appears evidently to be a purgative of the drastic kind; and as most purgatives in an over dose evince an emetic quality in a greater or less degree, so upon this supposition it is easy to conceive that this medicine may, in many cases, produce nausea and vomiting.

Now the treatment of gout by strong purgatives is by no means a modern discovery. The limits of a paper of this kind will not allow me to trace back the history of the practice: it will be sufficient to observe, that it prevailed pretty generally and successfully previous to the time of Sydenham, when unfortunately for science and the afflicted sufferer, that enlightened physician interdicted the practice, because it did not coincide with his ideas of morbific matter, and the necessity of its expulsion through the medium of the extremities. "It being," he observes, "an inviolable law of nature that the matter of the disease should be thrown out by the extremities; emetics and cathartics will have no other effect than that of bringing back the offending matter to the bowels."*

In another part of his treatise, we are told that the practice, however dangerous, was in high repute with certain empirics; he admits that during the purgative operation of the medicines administered, the patient felt no pain, or at least very little; and that if the catharsis could be kept up for several days, the patient was certainly cured of that paroxysm.†

* "Deinde, catharsin quod attinet, sive ἄνω sive κάτω animadvertendum est, quod cum naturæ lex sit inviolabilis, atque ipsi hujusce morbi essentiæ intertexta innexaque, morbi fomitem semper in articulos rejici debere; nihil prorsus aliud præstabunt remedia sive emetica, sive cathartica proprie dicta, nisi ut materia peccans, quam natura in corporis extremitates protruserat, in sanguinis massam denuo revocetur; unde accidit ut quæ in articulos eliminari debuerat, in aliquod e visceribus forte irruat, atque ita æger, qui in nullo prius discrimine versabatur, jam de vita periclitetur." SYDENHAM, Tractatus de Podagra.

† "At vero hæc ipsa methodus, utut perniciosa ac nocens, nihilominus empiricis quibusdam, qui catharticum quo utebantur medicamentum astute omnes celarunt, haud mediocrem estimationem conciliavit. Observandum est enim quod purgatione currente, æger vel non omnino, vel remisse admodum, dolet; & si catharsis ad plures dies produci queat, nullo superveniente paroxysmo recenti, æger confestim ab eo, quo jam tenetur, convalescet: verum enimvero pœnas in posterum pendet dirissimas ab ἀταξία, in quam dicta humorum exagitatio naturam præcipitem egit."

That this theory was framed in the face of personal experience, may be inferred from the latter part of the quotation, which amounts to an acknowledgment of the utility of the practice. That it was in opposition to the best practical authorities the world ever produced, will be evident to any one who will take the trouble to investigate the subject. Dr. Cheyne, says, "that in his time some eminent physicians had so little regard to the opinion of Sydenham in this matter, that in the fit of the gout itself *they never scrupled to drive it off*, both in themselves and others, by strong, quick, and active purges;" he further adds, "and most certain it is that this method will cure any fit of the gout, how obstinate soever, and that in a few days." We have also the authority of Kirkland for this practice, who asserts, that he has never yet seen under an inflammatory state, any method equal to purging for giving immediate relief; and this remark extends to the disease when it has attacked the stomach and bowels.

Scammony is a medicine which has been famed in all ages for its power of subduing the paroxysms of gout; and it is remarkable, that I have met with no author on the subject, whose prescriptions are not found to contain a large portion of this drug. The electuarium caryocostinum, the active material of which is scammony, was sold, in the time of Tissot, as a quack medicine in Geneva, under the title of opiate for rheumatism, and which he highly extols with some exceptions as to particular constitutions. Bate recommends a tincture of scammony as a medicine of great service in the gout, and the same tincture is, at this time, a popular remedy for that disease in France; and I am credibly informed that one gentleman for thirteen years past has, by means of it, succeeded in removing the paroxysms whenever they occurred.

Salmon extols scammony and elaterium as medicines which "*admirably help the* gout," and Turner's arthritic powder is a composition of turbith, hermodactyls, scammony, senna, and elder seeds, and was considered in its day as almost a specific for this complaint.

Willis recommends the electuarium caryocostinum and hermodactyls, which, he says, will make such as are not able to go, presently to walk about. The same medicine was before

prescribed by Alexander, with the same expressions, as to its virtues, so as to leave little doubt of its having been taken from that author.* He quotes Rodericus a Fonseca, in recommendation of the roots of black hellebore, and amongst other things an apple, with ʒss of its fibres stuck in it, which is to be roasted under the embers and eaten.

It would be easy to multiply authorities if it were necessary, but as they are within the reach of every inquirer, I shall merely add the following extract from Hoffman, which, from the information it contains, appears of too much importance to be omitted.

"Hisce deductis operæ pretium esse æstimo paucis judicium nostrum interponere de methodo hactenus usitata, sanandi dolores podagricos. Plures et ex iis exercitatissimi practici vomitoriorum usum non satis dilaudare queunt in affectu hoc, tam preservando quam sanando. Confert, ut podagrici omni verno et autumnali tempore circa æquinoctia preservationis causa assumunt vomitorium. In principio quoque podagræ, proficuum esse ipsi Galenici jamdudum testati sunt. Dolæus Encyclop. Med. p. 635. Hildanus, Cent. 6. Obs. 34. Mercatus de Morbis internis, L. 4. c. 18. Riverius praxi, L. 6. c. 1. Sylvius, pr. p. 153. Emeticorum usum præsentissimum judicant. Alii magni faciunt preservationis causa laxantia ante ipsum paroxysmum, vel singulis mensibus.

"Doctissimus Solenander in Cons. p. 78, scribet se in paroxysmi principio solere exhibere, magno cum successu, aliquod purgans; primum id edoctum ab Alex. Tralliano, l. xi., licet a multis hoc tempore reformidatus. Singulare esse scribit eruditissimus Pechlinus, Obs. 26, quod arthritici leviorem plerumque experiantur insultum, detracta ex primis viis materia. Novi, ait, Empiricum qui medicamento chymico, seu Chrystalli Lunæ essent seu Martis aliquod vitriolum, etiam in inveterato jam morbo postquam biduo ante paregoricum de-

* Alexander's prescription is composed of hermodactyls, ginger, cummin seed, pepper, anniseed, and scammony, which makes those who take it walk immediately.

"τυτο ευθεως βαδιζειν αυτοις ποιει."

ALEX. TRALL. IN LOCO.

disset, alvum magno cum fructu provocabat. Ex adverso Sydenham illorum usum penitus rejicit. Nostra sententia hæc est; qui plenius vescuntur, et largiori victui assueti sunt, et mox ante paroxysmum primum invitante appetitu plus justo indulserunt gulæ, appropriata cathartica vel emetica summo cum fructu atque emolumento assumere possunt. Depletis sic primis viis a copiosa saburra humorum et acidi superantis, leviores evadunt paroxysmi, nec tam molesta symptomata anxietates dolores patiuntur ægri, &c. In eandem sententiam nobiscum accedit Lister, Tract de Arthritide, qui ita loquitur, &c."*

In another treatise, de dolore podagrico et arthritico, he says, "Memini me olim statim sub initio doloris podagrici et arthritici, insigni sæpe cum fructu, usum fuisse sequentis mixturæ. Aq. flor. acaciæ ℥ij. Aq. cinn. sine vino ℥ss. P. corna-

* Dissertatio de genuino et simplicissimo podagræ remedio. Hoff. oper. vol. vi.—The sense of the author may be thus rendered:—"I shall say a few words on the method hitherto employed for the cure of gouty pains. Many of the most experienced practitioners speak in the highest terms of emetics in this complaint, both for prevention and cure. Gouty patients derive great benefit from emetics taken as prophylactic, at the vernal and autumnal equinoxes. In the commencement also of gout, the beneficial effects of these remedies have been long ago testified by the followers of Galen. Dolæus, Hildanus, Mercatus, Riverius, and Sylvius, consider emetics as a most effectual remedy. Others think highly of laxatives as a means of prevention before the paroxysm, or taken monthly.

"The learned Solenander writes, that he was in the habit of administering a purgative with great success, in the commencement of a paroxysm, that he first learned this practice from Alexander of Tralles, though by many it was much dreaded.

"Pechlin says, 'it is a singular fact that arthritics have a milder fit if the primæ viæ have been first cleared. I know,' says he, 'an empiric who, by a chemical medicine, whether it was the crystals of silver, or a vitriolate of steel, is uncertain, procured evacuation from the bowels with great success, and this even in an inveterate disease, he having given a paregoric two days before. On the contrary, Sydenham entirely rejects the use of purgatives. I am of opinion, that in those patients who are accustomed to eat much, and who just before the paroxysm have been too indulgent with their appetite, proper emetics or cathartics may be taken with the greatest advantage.

"The primæ viæ being thus cleared of the saburra of the humors and superabundant acid, the paroxysms become milder, nor do the patients feel so much pain or uneasiness. Lister, in his Treatise on Gout, is of the same opinion with me on this subject, and has these words, &c."

chini ʒj. Syr. rosar. laxativ. vel rhabarb. cum cichoreo ℥ss. M. prohaustu. A drachm of the pulv. cornachini contains, scammony, diaphoretic antimony and cream of tartar, of each equal parts, so that twenty grains of scammony was given for a dose.*

The experience of the present day is equally strong in favour of the practice.

Very early during the agitation of this question the attention of the profession was generally directed to the Elaterium as a medicine most likely, from its known effects, to be the basis of the *Eau Medicinale*. I determined, therefore, to put it to the test of experiment on the first patients I met with. Three cases soon presented themselves, in which the curative powers of this remedy were as speedy and as satisfactory as any thing that could possibly have been expected from the gout medicine itself.

It is deserving of remark, that independently of its purgative, emetic, and diuretic effects, Elaterium possesses another property in common with the gout medicine, that of producing a tingling sensation in the extremities of the fingers.

This effect, which was noticed in the year 1695, by Dr. Martin Lister, was first pointed out to me by Dr. Buchan, of Percy-street, and was very strongly marked in the persons to whom I administered it. Whether this property is only exerted in gouty patients must be determined by future observation; Dr. L. however, spoke of its effects generally in his Treatise on Dropsy.†

The first case which was submitted to its influence was that of a labouring man, John Tomkins, who had long been a sufferer from that form of the disease called by many rheumatic gout,‡ and was never entirely free from its attacks; he had

* "I remember having used the following mixture with great success in the commencement of gouty pains, &c." De dolore arthritico et podagrico. Hoff opera, vol. 2.

† Lister Exercitationes de Morbis Chronicis. Tract. de Hydrop. p. 50.

‡ Some practitioners call the acute rheumatism, rheumatic gout; the latter disease, which is here alluded to, is characterized by inflammatory swellings in the larger joints of the body, *with little or no fever*, whereas acute rheumatism is always attended with great heat of skin, and the other symptoms of fever.

been under the care of the most eminent hospital physicians in town, without deriving any benefit from their treatment. I recollect having attended him upon a former occasion under a severe illness from this complaint, when his life was despaired of for many days, so much was he reduced by its violence and duration; at this time, the stomach and bowels were much affected by metastasis, and he recovered under the use of large doses of volatile tincture of guaiacum.*

He had now inflammatory swellings of several joints, particularly of his hands, wrists, and feet; the knuckles were permanently enlarged, and the fingers in a state of contraction, from former attacks. The pain often subsided in one part for another; sometimes it went from the joints to the bowels, and his friends became seriously alarmed for his safety. I gave him half a grain of elaterium every two hours, and on the following morning I found him free from all his symptoms, but great debility, and complaining of the severe operation of his medicine. The disease returned the following day, and was again relieved in the same manner.

Within these few days, a period of nearly eight months having elapsed since his last illness, he has had another attack of the gout; but here the seat of the disease was not as in the other case in the extremities, but confined to the bowels. He had been ill nearly a fortnight, complaining of tightness across the bowels, and what he denominated a heavy pain; he had nausea, impaired appetite, and flatulency; his bowels were costive, and he was generally indisposed. These were considered to be symptoms of gout, affecting some of the abdominal viscera. Three grains of elaterium were ordered, with directions for him to take one half immediately, and the remainder at night. Having taken one portion of this medicine, in the evening he was attacked with the most violent pain in his bowels; the remaining portion was given him, and a purgative clyster with two drachms of aloes and the same quantity

* A more extended experience, since this time, has abundantly confirmed me in the opinion I had formed of the efficacy of this remedy when administered in large doses. I think I may assert with confidence, that there are few cases of gout which may not be subdued by means of it. The efficient dose is ℥ss or ʒvj taken in half a pint of water gruel, night and morning.

of jalap ordered, which however was not administered. On the following morning I found he had been in excruciating pain all night; in the evening I saw him, the tongue was of a reddish brown colour and parched, his breath offensive in a remarkable degree, of which no words can convey an adequate idea, but which is very commonly met with in this stage of the complaint. With these symptoms of evident danger I did not think it right to rely solely on the elaterium; three grains more were given, and ℥xvj of blood ordered to be taken away; he was easy for the space of three hours after the bleeding, when the pain returned; on the following morning he was in great pain, the medicines had not operated. The clyster was repeated twice this day without effect; in the evening he was still in acute pain, and was again bled; ℈j of calomel was given, with directions to have the clyster repeated, which however was omitted; he was much relieved by the bleeding, but on the following morning the pain returned; he had one stool in the night; half an ounce of volatile tincture of guaiacum with fifty drops of laudanum were now given, which produced ease for the whole of the day. Towards evening the pain returning, the medicines were repeated with equal benefit. On the following morning he was free from pain, and his bowels were open; he was directed to continue his medicines a day or two longer, and is now quite recovered.

In the other cases the disease was confined to the back of the hand and wrists, it had here existed many days; each had three grains of elaterium administered, which produced a strong purgative effect, but the symptoms were removed with the exception, in one case, of a trifling degree of pain still remaining, though it was not of sufficient importance to require a repetition of the medicine. In one of these patients it was remarkable that the pain ceased before the evacuation from the bowels took place; it is not improbable that the salutary operation of the medicine was occasioned by some change in the secretions of this viscus effected by it. The same result is sometimes observed from the eau medicinale.

These cases have been related to show the advantage of attacking this complaint by purgatives. The precursory symptoms of a paroxysm of gout; the inappetency, flatulence, sour

eructations, and constipated bowels, indicate a disordered state of the intestinal canal. If the village dame be consulted by a patient in this case, he will be told that his bowels are in a bad condition, that he must take a dose of physic; and, probably, the opinion of the medical practitioner will not be very different, if he has not the good sense to know that gout is coming on. But if his mind is warped by prejudice this knowledge is sufficient for the prohibition, and the unfortunate sufferer is consigned to patience and his flannels.

The *sicca alvus*, or torpor of the intestinal canal, is a very common attendant on this disease. The celebrated father of physic, who lived 360 years before Christ, thought that gout was curable by no human means in old men, with *constipated bowels* and nodosity of the joints. But the disease even in them was not proof against a providential attack of dysentery, which cured it readily. He found other profuse evacuations from the bowels (τηξις, which literally signifies a melting down) also very useful; and he tells us, that a young man whose bowels were readily acted upon, who lived regularly, and was fond of labour, had only to find a physician with common understanding, and he might easily be cured.*

The power of diarrhœa in carrying off a fit of the gout when seated in the extremities is so notorious, that it could hardly escape so accurate an observer as Sydenham; but blinded as he was by a prepossession for his favourite opinion, he could not be expected to derive that practical advantage from it which the fact was capable of imparting. We cannot, then, be surprised to find him giving solemn directions for its suppression, as the only means of recalling the wanderer to his proper seat.

"There is but one remedy," he says, "that I know of, viz. to produce perspiration, by the medicines usually given for this

* Ὅσοι μεν ἢ γερόντις, ἢ περι τοῖσιν ἀρθροισιν επιτωρωματα ἔχυσιν, ἢ τροπον ταλαιπωρον ζωσι κοιλιας ξηρας ἐχοντες ουτοι μεν παντες αδυνατοι υγιεες γινεσθαι ανθρωπινη τεχνη. οσον ἐγω οιδα· ιωνται μεν τουτυς, αριστα μεν δυσεντεριαι ην επιγενωνται, αταρ ϗ αλλαι εκτηξιες ωφελησι καρτα, αι ες τα κατω χωρια ρεπυσι· ος δε τις νεος εσι ϗ αμφι τοισιν αρθροισιν ηπω επιτωρωματα ἐχει, ϗ τον τροπον εσιν επιμελης τε ϗ φιλοπονος ϗ κοιλιας αγαθας ἐχων, υπακουειν πως τα επιτηδευματα, ουτος δη ιητρου γνωμην ἐχοντος επιτυχων υγιης αν γενοιτο. Hippocrates. προρρ. lib. 2.

purpose, which if effected for two or three days, morning and evening, two or three hours at a time, the diarrhœa is commonly stopped, and the gout comes *thundering* back upon the extremities."* It should be remarked, however, that when he has reason to believe the diarrhœa is critical, he does not attempt to interfere with it.

It is impossible for the imagination to contemplate the strength of the *thundering* figure, which he uses to express the return of gout to the limbs, without picturing to itself the triumphant ecstacy of the man on the seeming victory he had obtained; whilst his patients, writhing under the most insufferable anguish human nature is capable of bearing, are consoled with the cheering prospect of longevity, which he invariably calculates upon, *cæteris paribus*, in a certain proportion to the violence of the pain.†

How different is the conduct of Musgrave on this point, who, after telling us, that during a paroxysm of gout a diarrhœa often takes place, which carries off the pain and tumefaction,‡ adds, that "the diarrhœa which *anticipates* the fit is frequently found to be salutary, health and vigor returning after it.§ The event of it, however, he confesses is uncertain. If it stops in time and is not *excessive* it proves serviceable, by carrying off the gouty matter by a safe way, though not the most common; and it has this advantage attending it, that the paroxysm *does not return for a long time after*.‖ He displays

* "Unicum, quod scio, remedium est, ut sudor provocetur methodo et medicamentis huic usui destinatis: quod si fiat ad biduum triduumve mane et vesperi, per duas tresve horas continuas, sistetur ut plurimum diarrhœa, et morbi fomes magna vi in artus *detonabit*." SYDENHAM.

† Morbo jam discusso, ægri tum ὀυξια, tum appetitus, redeunt, prorata doloris quo sæviebat paroxysmus nuper elapsus, et in eadem proportione servata, vel acceleratur vel differtur sequens paroxysmus. Nam si hic ultimus ægrum *pessime multaverit*, sequens paroxysmus non, nisi anno ad idem punctum revertente, denuo accedet." SYDENHAM.

‡ "Accidit diarrhœa; eamque statim excipit doloris et tumoris diminutio." De Arthritide Anomala, page 137.

§ "Atque istiusmodi fluor paroxysmos interdum Arthriticos antevertens, salutaris esse reperitur. *Expulso etenim inimico, claudantur januæ; pax, et quies restituitur; viget œconomia*." Page 138.

‖ Hujusmodi diarrhœa incerti admodum eventus est; si enim tempori cohibeatur, neque modum excedat, utilis invenitur; et quidem arthritidis

the folly and danger of an attempt to stop it; "for an officious diligence," he says, "disturbs nature, and interrupts her in the work she has begun, when it is better to leave her to herself, and permit that to be discharged, which if retained would do mischief; but if it becomes excessive it should certainly be restrained."*

That the gout is symptomatic of the affection of the bowels, I have little doubt. Stahl endeavours to account for the alternation of this disease with hypochondriasis, which may evidently be traced to this source.† If we admit that erysipelas is often connected with sordes in the intestinal canal, we shall have little difficulty in conceiving that another inflammation, in some respects similar, may arise from the same cause.

The affinity that exists between these two diseases may be inferred from the fact of their frequently alternating with each other. After a paroxysm of gout has been fairly fixed upon a joint, in all regularity and form, and has continued there for several days, it is sometimes suddenly transferred to the face and neck, or other parts, where it appears in the shape of an erysipelas, and *vice versa.*

Hoffman relates the case of a patient who had an extensive erysipelas of the right leg, which soon seized the metatarsus and toe of the left side, with considerable tumefaction and pain. The surgeon, who was in attendance, applied spirit of wine and camphor to the foot, upon which it attacked the other: by the application of powdered camphor, the disease was again immediately relieved, but the patient was seized with symptoms of gout in the vicinity of the heart, with so great a constriction upon the diaphragm that he could hardly breathe.‡

materiam etiamsi via non usitatissima, tamen tuta emittere. Cui isthuc consequens, quod paroxysmus *non nisi diu post* recurrit arthriticus. P. 139.

* Nihil inepta sedulitate *stultius* dicam an *periculosius?* quæ, Pharmacis perperam ingestis, naturæ opus sæpe interturbet, interdum abrumpat. Suo potius arbitrio ea, patiaris, agat; quodque intus læsurum erat suo utiliter ingenio effundat, si vero diarrhœa *nimium excurrat*, viresque excedat, astringentibus sistenda est. Page 139.

† Stahl. Theoria Medica, page 1372.

‡ Observations Interessantes sur la Goutte et Rhumatisme, traduites de Fred. Hoffman. Obs. 2. 241.

Musgrave also speaks of the transition from erysipelas to gout in the joints;* but he is not warranted, I think, in assuming from this fact the existence of an arthritic erysipelas. The *morbi fomes*, the daily accumulations of indigested sordes, is the exciting cause of both these and of many other diseases. Gout in the joints, erysipelas in the skin, asthma, phthisis, peripneumony in the lungs, cholic in the bowels, commonly called gout in the stomach: these, and a variety of other symptoms from the same cause, may be removed by the evacuation of the offending matter, or by retunding and neutralizing its acrimony. His book is a most invaluable production, inasmuch as it shows what an endless variety of nervous and chronic diseases may be traced to this source.

If this opinion be established, the superiority of purging over bleeding will be evident; the latter may relieve, as in all cases of inflammation it does, but it is only by removing the cause that we can expect a removal of the disease.

With these facts before me, I cannot help recurring to the exploded doctrine of the humoral pathology, as affording the only plausible solution of the phenomena of this malady. Is it, I would ask, inconsistent with the known laws of the animal economy, to suppose that an acrid matter may be carried by absorption from the intestines into the circulation, and that its existence in the blood is incompatible with the healthy functions of the body? If I might be allowed to step beyond the limits of fair induction into the field of hypothesis, I would imagine the joints, from some peculiarity unknown to us, to be the parts chosen by nature for the deposition of this matter; or I see no difficulty in allowing its transpiration to be going on through every pore, whilst the joints or parts attacked, from this unknown peculiarity, are more susceptible of diseased impression from its acrimonious nature than others.

Pain is not an inseparable concomitant of this disease: its intensity, or indeed its very existence, is probably determined by the texture or sensibility of the parts attacked. Thus in the

* "*Faciei* porro *erysipelas* notavi quod detractione sanguinis, factaque inde materiæ in artus Διαδοχῇ quum in *dolorem* mutaretur isthuc *arthriticum*, naturam plane confessum est *arthriticum*, &c." Musgrave de Arthritide anomala, p. 459.

commencement of a regular paroxysm which is seated in the ligaments of the joints, acute pain is felt, but when at the close of each "*paroxysmulus*" the pain is commuted for swelling, erythematous redness, and itching of the surrounding integuments, it is not unfair to conclude that the matter has been translated from the ligaments to the skin. When the disease by metastasis or otherwise affects the brain, separate from its meninges, no pain is felt, but giddiness, stupor, or apoplexy are the consequence.

Cullen's fourth objection to the theory of morbific matter is, that its operation, if it exists, should be similar in the several parts which it attacks; whereas it seems to be very different, being *stimulant*, and exciting inflammation in *the joints*, but *sedative*, and destroying tone in *the stomach*.*

Lest any doubt should arise of the truth of this position, from the natural insensibility of parts of a ligamentous structure, I must be allowed in digression to observe, that however insensible in their healthy state, when they have taken on inflammatory action the pain is most *acute*. This arises from their dense, firm, and unyielding nature.

The acknowledged efficacy of blisters† in the cure of gout in many cases, when applied to the inflamed joint, strongly favours the theory of morbific matter. No person in the right possession of his senses would think of curing inflammation by means of blistering, unless with the hope of exciting a discharge of some irritating material producing the disease.

The local application of euphorbium,‡ elaterium,§ and me-

* First Lines, sect. 529.

† Και ἄλλον ὅμοίως ἐθεασάμην κεχρημενόν τῶ διὰ κανθαρίδων φαρμάκω, ϗ πυχαριστει τὰ μέγισα, ῥηγνυμένης γαρ της γινομένης ὑπὸ τῦ φαρμάκυ φλυκταινης ὑγρὸν ἐξεκρίνετο πολὺ ϗ τύτο συμβαίνοντος ἔφασκεν ὠφελεισθαι τὰ μέγισα.

"I have seen another patient who, by means of a medicine composed of cantharides, has been cured, for when the vesication occasioned by it was ruptured, there came forth a great quantity of humor, which he said relieved him greatly." ALEX. TRALLIAN.

‡ Δόκιμόν εσι τὸ φάρμακον ϗ ἐπι πολλῶν πολλάκις ευδοκιμῆσαν ἐπι τῶν τοιύτων διαθέσεων ϗ γαρ διαφορεῖ ϗ ἀμυσσει την εφιανειαν, ἕλκει τε ἐκ τῦ βαθυς τα ισρηνωμενα, ϗ διαλυει ϗ την οδυνην εκοπτει.

"It is a noble medicine, and frequently tried with success in this species of the complaint, for it excoriates and removes the cuticle, it draws forth that which is deep seated, and relieves the pain." ALEX. TRALLIAN.

§ "Radix elaterii ex aceto cocta podagricis illinitur succoque dentium

dicines of this kind, the beneficial effects of which are attested, the former by Trallian, the latter by Dioscorides and Pliny, can hardly be accounted for upon any other supposition.

Another reason in support of this theory, to say nothing of the relief demonstrably given by purgatives, is the immediate benefit arising from the use of those medicines which have the power of neutralizing acid secreted in the stomach and bowels. Half an ounce, or a larger dose of volatile tincture of guaiacum, given night and morning, is a remedy which from much experience I can recommend as a specific in many cases of this disease.

If the medical reader requires more evidence than his own experience will afford him of the existence of this acid, I may refer him, amongst others, to a case of gout related in the first volume of the Medical Observations and Inquiries, where a severe paroxysm was critically carried off by the vomiting of a considerable quantity of fluid, which from its acrimony was compared to strong mineral acid; added to this, the close analogy which subsists between some forms of dyspepsia in which the presence of acid is indisputably certain, and those symptoms which precede an attack of gout, leave little doubt of its existence also in the latter case.

In a systematic treatise much more might be adduced in support of this opinion; but as I fear I have already trespassed too long upon the patience of the reader, I must remain satisfied with having merely laid the foundation for future inquiry.*

dolori medetur." PLINII Hist. Nat. de Elaterio. Dioscorides is literally copied in this quotation.

* The objections to this doctrine first started by Stahl, and afterwards urged by the celebrated Cullen, are of little weight in the scale when opposed to the host of evidence which may be adduced in its favour. Cullen says, we have no direct evidence of the existence of morbific matter in the blood, and that previous to an attack of gout there appears no marks of any morbid state of the fluids. Granted: but we have strong proof of its existence in the intestines; and we shall have more difficulty in supposing the possibility of its continuance in the bowels without being absorbed into the circulation, than in accounting for the mode of its getting there. The arguments against the existence of this matter, deduced from the various and contradictory accounts of its nature, are too futile to deserve a moment's attention. It can never be deemed an objection to the truth of an opinion, that its partizans have taken wrong measures in its defence. Hence the attack upon the doc-

If we admit this fact, the dangerous consequences of an indiscriminate attempt to cure gout by the application of cold water will be obvious. That in many cases the powers of nature are proof against the destructive tendency of this practice cannot be denied, but that fatal instances have occurred in consequence of its employment is equally certain. Upon this point Trallian judiciously observes, "that no astringent or repellent medicines should ever be had recourse to, until the bowels have first been freed of the sordes contained in them."*

That gout is curable by medicine and proper attention to diet and mode of living, without danger of inducing other diseases, I have long been convinced by great attention to the subject; and so fully persuaded am I of this fact, that I should feel no hesitation in affirming that it is so in every case, if the experience of one individual could afford a sufficient warrant for such an assertion.

The administration of purgatives forms a prominent, though not the only feature in the medicinal part of its treatment. Their employment we have shown to be warranted by facts; they have the voice of reason on their side, for when "the enemy is driven from the citadel, and the gates shut, peace and tranquillity *must* be restored." They have the sanction of the most unquestionable authorities of antiquity for their utility; but that they are capable of removing the disease in every case, is more than I dare venture to affirm. Many cases may, no doubt, occur, in which they may not only be inefficient, but dangerous. The discrimination of the practitioner must here

trine contained in his 6th objection, founded on the mistaken inferences of its existence, from the hereditary nature and contagious property of the disease, is equally absurd with that last considered. The only objection which remains unanswered is, that the theory is inadequate to explain its frequent metastases from one part to the other. Is Cullen's theory unimpeachable on this score? No. Admitting then that we are not sufficiently informed respecting it to be enabled to explain *all* things *at present*, let us take that which we have as an earnest of that which is to come; and having nothing more plausible to oppose to it, wait with patience until more facts, or some newly discovered analogy, afford the complete and desired explanation.

* Ἐγὼ δὲ ῥηµι µηδὲ τοῖς ϛυφουσι χ ἀποχρυεσθαι δυναµένοις ἐπὶ τῶν πεπονθότων ἐθέλειν κεχρῆσθαι, µὴ πρότερον ὅλον ἀπερειττον ἐργασαµενον τὸ σωµα. ALEX. TRALL.

be exercised. Elaterium, though in a proper dose a *mild* medicine, will destroy life as well as the Eau Medicinale, and it is only by minute attention to the circumstances of the case, that its employment can be regulated.

As to the identity of any preparation of elaterium with Husson's remedy, it is, as I hinted above, of too little importance to contend for. Yet, I may observe, that the marked coincidence in effect between these medicines, speaks as strongly in its favour as medical evidence can speak. What pharmaceutical process it should undergo to render it similar in external appearance to that medicine, I shall leave to the consideration of those who are more curious in researches of this nature than I am.

This is a point at which no man of science need be ambitious of arriving. The research is endless. The time which has already been employed in the inquiry, and the varied speculations that have been entered into respecting it, with so little success, will almost justify this conclusion. It appears to me to be a prostitution of talents, implanted in us by the great Author of our being for wiser purposes; for we are seeking for that, which when discovered will add comparatively nothing to the stock of our information.

If I have shielded myself under the authority of men whose names are venerable in the annals of Medicine, I trust I have not suffered myself, by a blind adherence to them, to be drawn into the track where experience does not also lead. The remarks here made might have received support from cases under my own observation; but it is easy to make a tale suited to any theory, and the narratives of some modern writers are so much in opposition to the daily experience of all practitioners but themselves, that I was unwilling to risk an imputation so disgraceful to a man of science.

It is with reluctance that I have been induced to launch out into theory; contrary to my original intention, I have insensibly been drawn into it. If my opinions are opposed to facts, I shall be most happy to cancel them and to acknowledge my error. I have no object in this investigation but truth, and if I have not yet found her, she shall be a welcome guest whencesoever she may come.

JOHN WANT,

Surgeon to the Northern Dispensary.

July 23, 1811.

A Reply to some Observations and Conclusions in a Paper just published in the second volume of the Medico-Chirurgical Transactions, on the Nature of the Alkaline Matter contained in various dropsical Fluids, and in the Serum of the Blood. By GEORGE PEARSON, *M.D. F.R.S. Physician in ordinary to their Royal Highnesses the Duke and Duchess of York, and their household, &c. &c.*

[From the Philosophical Magazine, for January, 1812.]

To Mr. TILLOCH.

SIR,

I WAS favoured a few weeks ago, by Dr. Marcet, the author, with the above named paper. In it I have the satisfaction to find many of the facts confirmed, and none contradicted, which I have published in the Philosophical Transactions 1809 and 1810, on Expectorated Matter and Purulent Fluids; except with regard to the alkaline impregnations. My experiments informed me that expectorated matters and pus contain potash neutralized by an animal substance, or by an acid destructible by fire. I likewise found, as I prosecuted my inquiries, that there is the same kind of alkaline impregnation in the blood, in the dropsy fluids, in the fluid effused by vesicating with cantharides, in the fluid secreted from the nose owing to a catarrh, and even in the urine. And as I did not find the soda alkali in a similar state, I concluded that hitherto this alkali had, probably, been mistaken for the potash. (See our preceding numbers.) In the ingenious paper, however, which has occasioned this reply, it is asserted that the alkali in combination with the animal matter is the soda; but it is inferred that potash is also present, not in the state I discovered it, but united to muriatic acid.

It would not be treating the public justly, if I did not say that the paper before me contains an inquiry conducted conjointly by Dr. Marcet the writer, and Dr. Wollaston; as Dr. Marcet represents, I allow very fairly, to enhance the credit of his statement. Considering the power of these allied opponents, the odds are fearful; but confiding in the assurance of lord Bacon, that induction by experiment equalizes* the

* Nostra vero inveniendi scientias ea est ratio, ut non multum ingeniorum acumini et robori relinquatur; sed quæ ingenia et intellectus fere exæquet.—Bacon's *Novum Organum*, § lxi.

mental faculties among different men, I shall with this palladium obey the summons to the arena—at the worst issue, with such adversaries it would be glorious even to fall in the struggle:

——————— agimus proh Jupiter! ———
—— Causam; et mecum confertur Ulysses.

To enable the chemical public to judge rightly of the different conclusions, above declared, concerning the kinds and states of the alkalies existing in the animal fluids, the evidence of the opposing parties must be heard. The adverse party, however, have not attempted to invalidate my evidence, by showing that the conclusions are unjustifiable, but have merely exhibited their own experiments and conclusions. This mode of procedure, I apprehend, is not according to the laws of controversy; and it compels me to make a statement of at least some of the most decisive experiments for my conclusions, previously to the examination of the contravening evidence.

I. 961 grains of exsiccated sputum, on incineration and fusion, afforded 45 grains of saline substances consisting of 35 grains of cubical crystals of muriate of soda, and the rest were spiculiar and uncrystallized salt amounting to ten grains. These ten grains were separated for distinct examination. They manifested the properties of alkaline matter. On adding liquid tartaric acid to this alkaline matter also liquified, an effervesence ensued, with a precipitate of super-tartrite of potash only; "*certainly yielding no soda-tartrite of potash.*" With nitro-muriate of platina a grain or two of this saline matter produced a reddish precipitate. Now if muriate of potash, and carbonate or sub-carbonate of soda had existed, the result must have been soda-tartrite of potash and muriate of soda; or tartrite of potash and muriate of soda. This latter result is not so probable as the former, on account of the very large proportion of alkali to any other possible salt. The quantities, too, were obviously sufficient for producing compound salt determinable by the eye unassisted with glasses.

II. By digesting 2500 grains of desiccated sputum in four pints of alcohol or spirit of wine, the clear tincture decanted from off the undissolved matter afforded on distillation 140

grains of resin-like substance; which manifested no alkaline properties, but it indicated slightly acidity.

A portion of this resin-like substance being mixed with liquid tartaric acid was subjected to distillation; but neither muriatic nor any other acid was disengaged. This I conceive shows that no muriate of potash existed.

Twenty-five grains of this matter were acted upon by successive affusions of nitric acid; and on boiling to dryness and ignition, the deflagration which took place produced a charcoal-like mass containing potash. Hence the alkali had been united to something destructible by fire.

According to computation, the 140 grains of resin-like matter contained 28 grains of potash united to matter destructible by fire, and 18 grains of muriate of soda, with an inappreciable quantity of ammonia and phosphoric acid besides the animal matter. The matter undissolved by alcohol, in this process, afforded by incineration and fusion a mass consisting of 23 grains of muriate of soda with a very small proportion of potash mixed with 23 grains of phosphate of lime, traces of magnesia, iron, and a sulphate; also a minute portion of utterly indissoluble vitrified matter. If potash had existed in union with muriatic acid, it must have appeared in the fused mass left undissolved after digestion in alcohol: but potash did appear in a naked state after ignition and fusion of the matter dissolved in alcohol.

III. By digesting 4000 grains of sputum in two pints of rectified spirit of wine, the same results were obtained, excepting that the resin-like matter contained a much larger proportion of muriate of soda and animal matter.

IV. Twenty ounces of ropy sputum by digestion in ten pints of distilled acetic acid afforded, by evaporation of the clear liquid separated from the coagulated matter, a soft extract. This extract deliquesced, partially, on exposure for a few days to the air; but it manifested no properties of alkali. By exsiccation, ignition, and fusion of a little of this deliquesced matter, it afforded an aqueous solution which precipitated abundantly super-tartrite of potash on adding tartaric acid; and a reddish precipitate fell on the addition of platina solution. Almost the whole of this extract being exsiccated was digested in rectified

spirit of wine, affording a blackish tincture after evaporation to dryness, which became liquid by 24 hours exposure to the air. It was almost entirely acetate of potash. I believe acetate of soda neither dissolves in alcohol nor deliquesces; but, independently of these properties, the alkali united was proved to be potash.

I shall call no other evidence from a great mass which remains in my published papers. If I were to follow the example of my adversaries, I should also not trouble myself to examine their evidence; but as the question cannot be decided without such an examination, I beg permission to perform this duty.

I. *Of the Fluid of the Spina bifida.*

In the ten printed pages of experiments on this fluid by Dr. Marcet, I can only perceive that there is evidence for the existence of an alkaline subcarbonate; yet it is said, "Soda may be inferred from the effervescence with acids." The alkaline matter was treated with alcohol; and thus it was separated from the muriate. The alcoholic solution being decanted and evaporated to dryness, a residue supposed to consist of acetate of soda was obtained, which weighed between 17 and 18 per cent. of the mass." Oxymuriate of platina produced no precipitate.

I remark, that the first result only shows the presence of charcoal acid. 2. The acetate of soda is not, I believe, dissoluble in alcohol, but it is well known that acetate of potash is so. However, if there be the authority of experiment for the dissolubility of acetate of soda in this mentruum, still the experiment is equivocal. It was easy for the adverse party to have decided this question by the test of tartaric acid, provided there was an adequate quantity of matter for the trial.

3. I remark, that there being no precipitation with the platina solution seems to me to prove nothing; as the whole quantity of matter treated could not reasonably be supposed to amount to more than a small fraction of a grain; too small for the detection of potash by means of the platina solution, or even probably by the more sensible test tartaric acid, which was not used. Yet the ingenious writer has not only inserted

soda among the impregnating ingredients of the fluid under examination, but also boldy denoted the proportion to the centesimal part of a grain. I shall in another part of this communication, I believe, demonstrate that this analysis does not warrant the statement of the composition of this dropsical fluid given in such precise terms: for, on the ground of cogent analogy, I cannot doubt that one or more ingredients are present, but not inquired for by experiment, nor enumerated. Hence, not only is the analysis objectionable with respect to the ingredients but the proportions. It is true, in a subequent part of the investigation the deficiency seems to have been perceived and acknowledged; but if so, it will not be an easy task to justify the publication of perhaps an inaccurate analytical statement, in opposition to my experiments which have not been refuted.

II. *Of the Fluid of the Hydrocephalus internus.*

A few grains of the saline matter of this fluid consisted of cubic crystals mixed with spicular and opake globules. The assertion is several times made, that the spicular crystals and opake globules were carbonate of soda—that most of the cubes were muriate of soda; but some of the smaller ones were found to be muriate of potash. The proofs for the assertion are from the two re-agents I employed in the same inquiry; namely, tartaric acid and platina solution for the potash; and "the carbonate of soda was indentified, not only by tests indicative of the absence of potash, but also by its forming rhomboidal instead of prismatic crystals, when treated with nitric acid."

Now, I apprehend our judges will deem this evidence unsatisfactory; and that much more decisive proofs will be reasonably expected. I beg permission to ask, whether or not the laborious experiments upon a large scale, which I instituted, to exhibit evidence of the exclusive existence of the potash alkali, are to be disproved by the rhomboidal figure of the crystals, in place of prismatic, seen perhaps only by a magnifying glass in the quantity of a grain or two dispersed over a comparatively extensive surface; and whether or not the absence of potash, indicated by tests operating upon minute quantities, is unequivocal evidence, and ought to counterpoise

experiments, with quantities affording products, of which no doubt can be entertained. I do not question the accuracy; but I hope it is proper to take a further objection against the competency of the experiment asserted, for the presence of soda and absence of potash. On the most important point which occurred in the inquiry, the kind of alkali existing in the fluids, I do conceive that more experiments, and particularly detailed, are necessary to effect the dis-proof of what I have published, and to command assent, that soda and not potash is present. Is it satisfactory to affirm, that soda was indentified, because the tests did not indicate potash? It is quite superfluous for me to say to such learned adversaries as I have the honor of addressing, that an experiment might have been instituted to have afforded unquestionable proof of the existence of soda. Such a proof would be the composition of a binate salt, possessing the known properties of a compound of soda and the acid employed.

With respect to muriate of potash, that this is present, is supported only by the observation of smaller cubic crystals among larger ones; otherwise it is a mere assertion.

My last argument is of a different kind from those above stated. If carbonate of soda in a large, and muriate of potash in a small proportion be present, on the addition of tartaric acid, it is obvious that it is scarcely possible to avoid compounding soda-tartrite of potash, and certainly muriate of soda. If the learned opponents had produced these compositions, I must have conceded, at least, that carbonate of soda existed; but still it would require other experiments to determine the state of the potash.

III. *Of the Fluids of Ascites, Hydrothorax, and Hydrops Pericardii.*

A saline mass amounting to 4·8 grains, obtained by the process above mentioned, exhibited clusters of crystals, partly cubic, partly octohedral, interspersed with others of a feathery or radiating appearance. The feathery saline matter effervesced briskly with acids, and yielded no permanent precipitate, either with tartaric acid or with oxymuriate of platina. The cubic crystals

and octohedral yielded precipitates with either of the two tests just mentioned.

I do not conceive that these observations authorize the adverse party to contravene my experiments and conclusions. I know from experience, that it is probable the feathery crystals even of potash would elude detection, on account of the minute quantity; there was however a precipitate, but not permanent. The question naturally arises, What was that non-permanent precipitate? I have no doubt the quantity was too small to enable the question to be answered even by the hands that performed the experiment.

But the cubic and octohedral crystals yielded precipitates with either of the two tests; and hence potash is inferred to exist united to muriatic acid. I again must appeal to chemical judges, to determine whether or not the conclusion is warrantable; for, 1. Here is no proof of muriate of potash. 2. It is not even certain that the precipitate was super-tartrite of potash. 3. Granting that super-tartrite of potash was produced, it remains to be proved in what state this alkali subsisted.

IV. *Of the Serum of the Blood.*

The saline matter procured from this fluid did not, with the platina solution, "produce a precipitate sufficiently distinct to be conclusive as to the presence of potash; but, by means of tartaric acid, a distinct though not abundant precipitate was produced." Further, with nitric acid this saline matter yielded crystals of a "rhomboidal form." Again: this matter dissolved in acetic acid, being evaporated to dryness, was treated with alcohol and again evaporated: "the residue, contrary to my expectation, exhibited traces of potash; but the same residue, with nitric acid, yielded rhomboidal, and no prismatic crystals were seen;" whilst "potash was easily discoverable in the residue, which had now lost its deliquescent quality." I wish to avoid repetition of objections already offered, although they are applicable in this place, and will only remark: 1. That I cannot admit the figure of such minute crystals, as a decisive property; but the kind of nitrate compounded might have been ascertained by the test of tartaric acid. 2. The dissolution of the

acetate in alcohol is the most conclusive experiment given in the paper before me; and it has produced apparent embarrassment. Even as performed it is pretty determinate, and might have become an *experimentum crucis* by prosecuting it a little further. We know that acetate of potash is dissoluble in alcohol, and there is no proof that soda united to acetic acid is present; even if such a compound be dissoluble in alcohol. It has been thought right, however, to assume an hypothesis, or more truly two hypotheses, to account for the potash in the menstruum of alcohol; viz.—To imagine that muriate of potash is present. 2. That it is dissoluble in alcohol. If potash was present in the indissoluble residue, it was most important to have exhibited the state in which it existed. It was not difficult to determine, if doubted, the state of the potash in the alcohol, by burning the residue left on evaporation, which would have denuded it if united to the acetic acid, but not if united to the muriatic acid. Supposing it be judged right to receive these experiments as evidence of the facts asserted by the adverse party, I beg to claim the right also of opposing the contravening evidence above delivered, in stating the results of similar experiments.

From this representation, I submit to our judges, whether or not I am entitled to object to the enumeration of subcarbonate of soda as one of the impregnating ingredients of serum; and especially to the proportion denoted in centesimal parts of a grain, in a mass amounting to seven or eight grains, consisting of seven different substances. Having communicated merely the information of the senses* through the intermedium of experiments, the chemical world will determine whether or not the opposing party have demonstrsted errors in observation or illegitimate conclusions. I am of opinion, that the best founded conclusions are but provisional, and of course, that chemistry has not yet attained the rank of a science, or at least not of a demonstrative science. This opinion seems just, from

* Sensus enim per se res infirma est et aberrans; neque organa ad amplificandos sensus aut acuendos multum valent; sed omnis verior interpretatio naturæ conficitur per instantias et experimenta idonea et apposita; ubi sensus de experimento tantum, experimentum de natura et re ipsâ judicat.—Bacon's *Novum Organum*.

a retrospective view of the varying states of chemistry during the last hundred years. Many of the theories of the illustrious Stahl were for half a century admitted as demonstrations of the agency of phlogiston. That these doctrines were erroneous, was evinced by the succeeding discovery of the agency of oxygen, especially manifested by the ever to be lamented Lavoisier. And the pneumatic doctrine in some parts has lately been rendered doubtful, if not exploded, by the wondrous achievements of professor Davy. Contemplating the prospect of the progression of this branch of natural knowledge, I offer the conclusion, that potash, and not soda, is the alkali united to animal matter in the fluids I examined, merely as provisional. That potash does also exist in these fluids, united to muriatic acid, is not inconsistent with my experiments; but the experiments of my learned friends do not appear to authorize such an inference. The discovery, however, will be partly due to them if hereafter the fact be substantiated.

I cannot close this communication until I shall have said a few words concerning the high encomiums on microscopic chemistry, accompanied by the bitter philippic against "the dismal, large, subterraneous laboratory." Chemistry must now, we are told, be transferred to "the comfortable fire-side of the drawing-room;" from Vulcan's foul stithy to my lady's chamber. This *elegant* change is to give "new impulse to the advancement of the science, and new schools are to rise under new auspices." Most happy shall I be to find these Eutopian prospects realised. It seems, however, more than probable, that the successful impulses already given by the schools of "my very learned and approved good masters," Cullen, Black, and Fordyce, will retain the cultivators in the paths now opened. And with regard to the scene for operations, the privilege of *taste* will be asserted; for that indeed is not disputable either in chemistry or elsewhere. Becher's taste was opposite to that of the ingenious new advocates: "Nec quicquam præ carbonibus, venenis, fuligine, follibus, et furnis valere potest."—*Phys. Subter. Præf.* The lord high chancellor of England not long ago declared in court, that he would not pay *sixpence* for the rapturous notes of Mara or Catalani. This also was a matter of taste, and no one disputed: it was only observed by a large

majority, that his lordship had "no music in his soul, and was not charmed by concord of sweet sounds."—No more.

The value of a tree is best known by its fruits: and accordingly to inform the judgment of the public by practical examples, and as some return for the notice with which my papers have been honored, I shall, with your permission, offer for your next number a few remarks on the publication in general which has produced this communication; in which, whatever differing opinions may subsist, I assuredly must admire the ingenuity, and respect the knowledge, of the honorable antagonists.

G. P.

George-street, Hanover-square,
Jan. 14, 1812.

Experiments on Muriatic Acid Gas. By J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

[From Nicholson's Journal for February 1812.]

TO MR. NICHOLSON.

SIR, Edinburgh, December 28, 1811.

THE state of my health has not allowed me to send you an earlier account of the further experiments on the nature of muriatic and oximuriatic acids, which I announced in my last communication. I now beg leave to submit them to the attention of your readers.

I have already observed (Journal, vol. xxviii, p. 139,) that there are two modes of investigation, by which the question at present under discussion with regard to the nature of the relation between muriatic and oximuriatic acids may be determined. Either it may be shown, that oximuriatic acid does or does not contain oxigen; or it may be proved, that muriatic acid gas does or does not contain water.

If it be proved, that oximuriatic acid contains oxigen, then it must be regarded as a compound of that element with muriatic acid, and the discussion is at once terminated.

The other mode, though less direct, is equally conclusive. In the experiment of the mutual action of oximuriatic gas and

hidrogen gas, muriatic acid gas is the sole product. Mr. Davy regards it as a compound formed by their union; and, if it can be shown to be the real acid free from water, or any other ponderable matter, this is the conclusion, which appears necessarily to follow. But, if muriatic acid gas contain water, the conclusion is inadmissible; the origin of this water must be accounted for; and there is no other mode of doing this, but by the established theory, that oximuriatic acid is a compound of muriatic acid and oxigen; and that, in its action on hidrogen gas, its oxigen combines with the hidrogen, forming water, which the muriatic acid, its other element, holds combined with it in the gaseous form. The proof therefore of the existence of water in muriatic acid gas is a conclusive proof of the truth of that theory, and at the same time a demonstration of the falsity of the opposite hypothesis. My former experiments were designed to gain proof of the existence of oxigen in oximuriatic acid: those which I have now to state were undertaken with the view of obtaining evidence of the existence of water in muriatic acid gas.

The difficulty is to find in this mode of investigation an experiment, which shall be conclusive. Such is the facility with which both hypotheses may be adapted to the phenomena, that there is scarcely a case of chemical action exerted either by muriatic or oximuriatic acid, in which an explanation may not be given in conformity to the one as well as to the other. And although the explanations afforded by the common system are less complicated than those of the other, and are more conformable to analogy from similar cases of chemical action exerted by other acids, yet still, since a possible explanation may be given by the latter, the question remains so far undecided.

This observation applies to the experiments, from which it was inferred, that water exists in muriatic acid gas; though at first view they appear to prove it, the proof must be admitted to be doubtful, as they admit of explanation on the opposite opinion. Thus the proof from the agency of water in facilitating the expulsion of muriatic acid gas from dry muriates is ambiguous, as the water may be supposed to operate either by its affinity to the acid, or by affording hidrogen to form it.

The production of hidrogen, when metals are acted on by muriatic acid gas, is a proof of equal ambiguity; since it may be supposed to be derived either from the decomposition of the acid, or of water existing in it. Even apparently the most conclusive of all these facts,—the production of water, when muriatic acid gas is acted on by substances with which acids in general combine, as for example the metallic oxides, admits of this double explanation. The acid is absorbed; and it might be inferred, that it combines with the metallic oxide, while the water which appears is deposited from the gas, in which it had previously existed in a state of combination. But this conclusion, though conformable to the most extensive and strict analogy, is avoided on the opposite hypothesis of muriatic acid gas being a compound of oximuriatic acid and hidrogen, by the supposition, that the acid is decomposed, that its hidrogen combines with the oxigen of the metallic oxide, and forms this water, while the metal itself combines with the oximuriatic acid.

If we can procure however a substance not oxidated, and yet capable of combining with muriatic acid, this source of ambiguity is avoided, and the experiment may be rendered conclusive. There is only one such substance—ammonia. No oxigen can be detected in its composition, and Mr. Davy himself admits, that it combines directly with muriatic acid, and does not decompose it. It cannot therefore cause any formation of water. Neither can it be supposed to afford water; for, when dried by exposure to substances having a strong affinity to water, it retains no sensible portion; nor is any discovered to have existed in it, when it is decomposed. Its combination with muriatic acid gas is thus calculated, I conceive, to afford what is so desirable, yet difficult to attain in the present question, an *experimentum crucis*. If, on combining dry ammoniacal gas with muriatic acid gas, no water is obtained, the result is so far in conformity to Mr. Davy's theory; and it may be concluded, that the water obtained in other combinations of muriatic acid gas has not pre-existed in it, but is directly formed. If, on the contrary, water is obtained; as it does not pre-exist in the ammoniacal gas, and as there is no such mode of accounting for its production as in those cases where oxigen is present, the water must be inferred to have existed in the muriatic acid

gas, and the truth of the common opinion is of course established. To ascertain the fact the following experiments were made.

Ammoniacal gas was dried carefully by exposing it over dry quicksilver to the action of quicklime. Muriatic acid gas received over dry quicksilver was combined with it, to neutralization: or rather leaving a very slight excess of alkali, to guard more effectually against any excess of acid, which might communicate to the product a slight degree of deliquescence. Thirty cubic inches of muriatic acid gas, and thirty-two cubic inches of ammoniacal gas, were employed. The white spongy salt was collected from the sides of the jar. It gave indications of humidity: for, although the surface of it appeared loose and spongy, it could not be entirely detached from the glass, but adhered slightly to it; in removing it by a knife, it spread a little over the surface, as any substance very slightly moist and clammy would do; and, when pressed together by a knife, its parts adhered slightly. It was put immediately into a small glass retort with a long neck, which was connected with a small receiver having two tubulatures, into one of which the tube of the retort was fitted by grinding, and into the other a long straight tube of narrow diameter, open at both extremities, was inserted. The retort being placed in sand, heat was applied by a lamp. In a short time a thin film of moisture condensed in the neck of the retort, which increased and collected into small globules, which accumulated, and trickled down: the heat being applied gently, that the salt itself might not be volatilized, there was no sensible condensation of humidity on the sides of the receiver, or in the tube inserted into it. When the further condensation of moisture appeared to have ceased, the lamp was withdrawn, the retort was cut, and the residual salt removed; a little of it, which had been volatilized, and formed a thin film on the upper part of the retort, being collected, and added to the other portion. The salt had lost in weight 1.3 gr.—a loss obviously to be ascribed to the expulsion of water, and the quantity condensed in the neck of the retort appeared fully equal to this. This is the smallest portion too, that was obtained in frequent repetitions of the experiment, and in some of these the quantity was equal to 1.5 gr.; a difference depending probably on the temperature applied.

100 cubic inches of muriatic acid gas weighing 39 grains, 30 cubic inches weigh 11.7 gr.; and this affording 1.3 of water gives the proportion of $\frac{1}{9}$ of its weight.

It could not be presumed however, that in this experiment the whole water of the compound salt was disengaged. In every case of the combination of an acid with any base, part of the water of the acid enters with it into the combination, at least when the product is a soluble salt, and is not easily entirely abstracted. There is no reason to suppose, that this should not be the case in the combination of muriatic acid and ammonia; and there must be even a greater difficulty in expelling this water from an ammoniacal salt by heat, than from other salts, on account of its volatility. There is another difficulty in the present case; we cannot introduce the affinity of any other substance to the acid, which, combining with it, might allow a portion of the water to be disengaged; for we can employ no substance with this view, but one which is oxidated, and which would therefore introduce a source of ambiguity, as it might be supposed, on Mr. Davy's hypothesis, to *form* water by its action on the acid itself.

The most direct method of discovering any further portion of water in the salt, free from this ambiguity, appeared to be to expose it to a red heat in mixture with charcoal; for, although the whole quantity could not be expected to be abstracted even by this mode, yet a portion might be expelled at so high a temperature, and the charcoal might also by its strong attractions to the elements of water abstract a portion, which would be indicated by the production of its compounds with these elements. The following experiment was accordingly made.

Charcoal in powder was exposed in a clear iron tube, the open extremity of which terminated in quicksilver, to a heat gradually raised to a very high degree of intensity; and this was kept up until the production of elastic fluid ceased. The charcoal was allowed to cool in the tube without the admission of air, and, when nearly cold, the salt remaining in the former experiment was mixed with about an equal weight of it. This was put into a Wedgwood's earthenware tube; the tube was nearly filled with the same charcoal, and was placed across a

small furnace, and surrounded with burning charcoal, so that the middle of it was raised to a red heat. A sufficient heat was thus communicated to the closed end of the tube to volatilize the ammoniacal salt, and cause it to pass through the ignited charcoal; to the other extremity a bent glass tube was adapted, terminating under an inverted jar filled with mercury in the mercurial trough. Elastic fluid began to come over; this was accompanied with a condensation of water in the curved glass tube; the gas itself very soon came over opaque, and humidity appeared on the sides of the jar, and the surface of the mercury within it. When two jars, containing about 14 cubic inches, had been filled, the gas which came over had become transparent; from 15 to 20 cubic inches were produced. Portions of this elastic fluid exposed to limewater caused a milkiness in it, with diminution of volume; the residual gas, after slight agitation with water, burned with the faint yellow flame of hidrogen, and, after its combustion, rendered limewater slightly milky. The charcoal in the tube being agitated with water, the liquor filtered from it was limpid, it had a strong saline taste, and on the addition of potash or lime exhaled a strong ammoniacal smell.

The rationale of this experiment is sufficiently obvious. From the temperature being much higher than in the preceding experiment, an additional quantity of water was expelled from the muriate, its separation being aided by the mechanical effect of the charcoal, which, while it impeded the sublimation of the salt through the whole length of the tube, would allow the more highly elastic watery vapour to pass. At the same time a portion of this water suffered decomposition, producing, by combination of its elements with the charcoal, carbonic acid, and carburetted or oxicarburetted hidrogen gas. The quantity of carbonic acid was from 1 to 1.3 cubic inch, estimated from the diminution of volume.

In both these experiments then, or rather in these two stages of the same experiment, the presence of water in the compound formed by the union of muriatic acid gas with dry ammoniacal gas is demonstrated. Its disengagement from the salt in the first stage of the experiment was not in the least ambiguous, and the quantity was even considerable in relation

to the quantity of acid gas employed, being equal to a ninth of its weight. And, as has been already remarked, this cannot be supposed to be the whole. Had there been no sensible production of water, the presence of any in the gasses combined could not have been inferred; and it could not therefore have been inferred with certainty, that any existed in the concrete salt. But since water was produced, and its existence in one or both of the elements of the salt is thus demonstrated, it is further certain, if we can rely on any conclusion from the most strict and extensive analogy, that the whole quantity could not be expelled by the heat applied.

In the second stage of the experiment, the disengagement of a further portion of water was abundantly evident, though from the nature of the experiment, it was difficult to ascertain its quantity with the same precision. Judging from the appearance of the condensed moisture in the curved glass tube, and in the jars, the quantity was nearly equal to that condensed in the first stage of the experiment; and to this is to be added the quantity decomposed by the ignited charcoal, which formed the carbonic acid and carburetted hidrogen. Adding these, and taking the average of the experiments, I would not hesitate in estimating it equal to the quantity, which appeared in the first stage of the experiment. This, supposing it to be derived from the muriatic acid gas (and, as has been shown, this can be the only origin assigned to it,) gives 2.6 of water in 30 cubic inches, or 11.7 grains of the acid equal to $\frac{2}{9}$ of its weight. The quantity of carbonic acid, (and this could be estimated with accuracy,) taking it at one cubic inch, contains as much oxigen as is contained in ·5 gr. of water, and this of itself added to the quantity obtained in the first stage of the experiment makes the water amount to $\frac{1}{6}$ nearly of the weight of the acid; with the addition therefore of the moisture visibly condensed in the tube and jars, the quantity cannot be less than between a fourth and fifth of its weight.

It may be remarked too, that though the quantity obtained in this stage of the experiment may not admit of being estimated with perfect precision, there is no source of fallacy with regard to its production. The charcoal had ceased to give out gas at a heat of much higher intensity than that to which it was

afterward exposed in mixture with the muriate; the water therefore, or the elastic fluid obtained, could not have been derived from it; and indeed this water appeared at the very commencement of the experiment, when the heat was scarcely equal to that of ignition. If the charcoal afforded any gas too, it could only be a portion of the carburetted hidrogen, and on the quantity of this produced no stress has been laid in drawing the conclusion from the experiment. And it is to be repeated, that the existence of water in the muriatic acid gas to the extent at least of $\frac{1}{9}$ of its weight is demonstrated in the first stage of the experiment, and that, from what must remain in the compound salt, the quantity must be greater than this.

Not only is the presence of water demonstrated by this experiment, but the quantity is nearly the same as that indicated by the action of other substances, which are supposed by Mr Davy to form it by affording oxigen. Thus Gay-Lussac and Thenard have inferred from the action of oxide of silver or of lead on muriatic acid gas, that it contains very nearly a fourth of its weight of water; and the quantity, which may be fairly inferred from the preceding experiments, is nearly the same.* Although it is not necessary, that the quantity should be proved to amount to this, to refute Mr. Davy's hypothesis, and establish the common theory, yet it is satisfactory to have this coincidence. And it must be further admitted as a proof, that the oxigen of these oxides has no share in the production of this water: for it is obvious, that, were the water, which is deposited when muriatic acid gas acts on metallic oxides, on the fixed alkalis, or the earths, formed by the oxigen of these substances, and not derived from the gas as previously existing in it, there can be no production of it in the mutual action of muriatic acid gas and ammonia, as ammonia cannot afford oxigen. Since it is produced in that action it must be derived

* The estimate by Gay-Lussac of the quantity of water in muriatic acid gas being equal to 1-4th of its weight is inferred from experiments, in which the product of the combination of the acid with the base is insoluble, and appears to have no affinity to water, as muriate of silver or of lead. It may be inferred, therefore, to retain little or none of the water of the acid, and hence the production of water to the amount of 1-5th or even 1-6th of the weight of the acid, in an experiment where the product must retain a portion of the water combined with it, is a near coincidence.

from the muriatic acid gas, and the same origin must be assigned to it in the other combinations of this acid.

This experiment then has the advantage of being conclusive on the subject of the present discussion; the state of the fact only requires to be ascertained, and with due precaution this is not difficult of attainment. There is at least no mode of accounting for the production of water, but by assumptions so gratuitous and unfounded, as to be equal to the refutation of the theory. Such is the only assumption that can be made—that the water may be derived from the ammoniacal gas, and not from the muriatic acid gas. When ammoniacal gas is dried by potash or lime, no water can be discovered in it by any test, nor is there any fact which affords a presumption that it contains water; the supposition therefore that it does would be purely gratuitous, obviously advanced to support an hypothesis. But further, dry ammoniacal gas is resolved by the action of electricity into hidrogen and nitrogen gasses; there is no deposition of moisture, and there is no intermixture of oxigen, as there must be were the water decomposed by the electricity. If the water obtained in the preceding experiments were supposed to be derived from the ammonia, it must therefore be maintained without any proof; it is contrary to all probability, that these gasses, which have scarcely any sensible attraction to water, should be capable of holding in solution the large portion indicated by the experiment. And if recourse be had to the hypothesis of unknown quantities of water in gasses, and if all these assumptions are to be made without any proof, are there not much stronger reasons for admitting its existence in muriatic acid gas, the affinity of which to water is so strong? It is obvious however, that were assumptions so numerous and gratuitous to be admitted in defence of an hypothesis, no experiment in chemistry could be rendered conclusive. That the water obtained in this experiment can have no such origin is further apparent from comparing the quantity of it with the quantity of ammonia. The specific gravity of ammoniacal gas is to that of muriatic acid gas as 60 to 124, or it is less than one half. In combining them about equal volumes were employed. Since the quantity of water obtained was equal to at least $\frac{1}{5}$ of the weight of the acid gas, it is equal of course to

$\frac{3}{5}$ of the other. If that water then were supposed to be derived from the ammoniacal gas, and on Mr. Davy's hypothesis it would be necessary to suppose the whole of it derived from this source, we must suppose, that, after being dried, this gas contains nearly half its weight of water. Yet no portion of this can be discovered in it, nor even detected when it is resolved by decomposition into its elements, hidrogen and nitrogen gasses. To add any illustration to this would be superfluous.

The statement of some additional experiments on this subject, and of a few experiments likewise on some of the compounds, as Mr. Davy regards them, of the oximuriatic principle with metallic bases, I must, from the length of this, reserve for another communication. I am, with much respect, your most obedient servant,

JOHN MURRAY.

P. S. In stating in my last letter, that the result I had observed, of dry oximuriatic acid gas not acting on carbonic oxide gas, was confirmed by the very same result having been obtained by Gay-Lussac and Thenard, I ought to have added, that it had also been obtained by Mr. Davy. In his account of "a combination of oximuriatic gas and oxigen gas," he states, among other properties of oximuriatic gas prepared in its pure state, that "it does not act on nitrous gas, or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried."* That Mr. Davy does not state this on the authority of others is evident, not only from the manner in which the sentence is expressed, but also from this, that he is giving an account of the properties of this gas in its state of purity, in which state there was no certainty of its having been obtained in former experiments, as chemists were not aware, that it might have an intermixture of oxigen, by which its properties and chemical agencies are materially modified. He gives this as a property of the *pure* gas, and of course he would not have done so without having ascertained it.

* Phil. Trans. for 1811, p. 156; or Journal, vol. xxix, p. 269.

SELECTED REVIEWS.

Relation Historique et Chirurgicale de l'Expédition de l'Armée d'Orient, en Egypte et en Syria. Par **D. J. Larrey**, *Docteur de l'Ecole Spéciale de Médecine de Paris, Chirurgien en Chef de l'Armée d'Orient, &c. A Paris, Chez Demonvelle et Sœurs.* 8vo. 1803. pp. 480.

[From the London Medical Review for July 1811]

THE length of time that has elapsed since the publication of the volume before us, and the extracts that have already been presented to the public, do not discourage us from laying an analysis of it before our readers, when we consider the high professional, and high public importance of some of the facts which it contains, and the small number of copies of the work that have yet reached this country.

As soon as the expedition into Egypt was decreed by the Directory, M. Larrey was appointed surgeon in chief, and M. Desgenettes (who in his commission is styled Médecin en Chef de l'Armée d'Angleterre) physician in chief to the army of the East, as the French were pleased to entitle it, which was to sail under the command of Bonaparte. To these two gentlemen was confided the whole direction of the medical arrangement of the expedition. They received from government a discretionary authority, not only to provide themselves with whatever they might desire for the completest furnishing of their respective departments, but to make up according to their own judgment, the medical and surgical establishment of that army. They applied accordingly to the professors of the schools of medicine at Montpellier and at Thoulouse, specifying the number and qualifications of the assistants they required, and desiring them to furnish them. In consequence of this application, M. Larrey procured one hundred and eight surgeons, exclusive of those regularly attached to the regiments. M. Desgenettes found more difficulty in supplying himself with a sufficient number of physicians. He applied to the school of Montpellier for six, expecting the complement to be made up by the army physicians; but the greater part of

these disappointing him, he was obliged to make a second application at Montpellier for as many more. The number ultimately under his orders he does not specify, but they were not fewer than seventeen, since he accounts for that number in the course of his work. Such was the medical establishment of the army of the East, amounting, according to Larrey and Desgenettes, to 30,000 men.

On the arrival of the army in Egypt, and the capture of Alexandria, having found an hospital in that town, he arranged his medical staff in six divisions, one being attached to each of the five brigades of the army, and the sixth, which he calls a corps de reserve of surgeons, being stationed under his own immediate direction, at headquarters. And afterwards, at Grand Cairo, and in every place of which the French took possession, hospitals with a proper medical establishment were formed. But besides these fixed hospitals, (*sédentaires*, Larrey calls them,) there were attached to the army camp-hospitals, as they may be called, (*ambulances* is his name for them,) which accompanied it in its marches, and in the field. During engagements they were disposed along the line in the most convenient situations for giving immediate assistance to the wounded; those whose wounds required immediate and capital operations being carried to the *ambulance* of the centre, where Larrey himself was stationed. At the first battle of Aboukir, in which the Turks were defeated, the French had 800 wounded, besides four generals, among whom were Lasnes and Murat.

"The most severe wounds," says M. Larrey, "were brought to the *ambulance* of the centre; I dressed them myself, and performed the necessary operations. Forty amputations were performed immediately, and with wonderful success. The wounded received on that occasion, from the surgeons belonging to the regiments and from the *ambulanos*,* the readiest and most efficacious assistance. *No one had to wait more than a quarter of an hour before his wounds were dressed.*"

At the battle of Alexandria, in which the French were defeated by the army under Sir Ralph Abercrombie, on the 21st of March, they had 1300 wounded; six hundred had been

* Surgeon attached to the staff, and not any particular corps.

wounded in the preceding actions of the 8th and 13th of the same month; eighteen hospitals were filled with them.

"The greater number were dressed, and had the necessary operations performed on the field of battle, or immediately after they were conveyed to the hospitals at Alexandria. All on whom amputation was performed within a few hours, were cured in a short time, and without untoward symptoms."†

No account we believe has been published of the fate of our wounded after the battle of Alexandria; but we know, from certain authority, that though victorious, many of them fared worse than the conquered. Although, in some instances, they received immediate assistance in the field, yet they were removed, soon after they had been dressed, or had undergone operations, to the fleet; and as the number of wounded far exceeded the means of accommodation that had been prepared for them, much misery ensued, and many lives were lost which with due care and assistance, might have been preserved. Orders were sent for several transports to receive as many wounded as they could contain. They were soon crowded, and boats, loaded with wounded, were seen rowing about in Aboukir-bay, for some time before a ship was found that could receive them; and when at length they were on board, there was, in some vessels, a total want of medical aid, as the number of staff-surgeons was not sufficient to supply the ships. It is true that the wounded derived great benefit from the attention of the navy surgeons; but, as some of these gentlemen were obliged to return to their own ships in the evening, many unfortunate men were left during the night without that attendance which their situation required.

We are aware that misery is inseparably connected with war, and we do not mean to censure the individuals who compose the medical establishment of our army. The fault does not lie with them; they are willing and able to do their duty. But where the medical officers are so few, where regiments on actual service have not always their complement of surgeons, how is it possible that the immense number of wounded that are brought to their quarters can be properly attended to? We do

† Page 435.

not know what difference there is in the number of regimental surgeons in the British and French service: but we have strong reason to believe (though we do not speak from positive authority) that the proportion is far greater among the French. And we should be inclined to trace, in the greater attention which the surgeons among our enemies have it in their power from their superior number, to give to their patients, the cause of a prejudice not common among soldiers and sailors in favour of the French surgeons, and which we have known to extend even to officers.

One great disadvantage of the insufficient number of medical officers is apparently that their work cannot be done in time; which in most cases of serious operation, means, that a life which might have been saved is lost. In speaking on one occasion of wounds of the limbs, from musket and cannon balls, which require amputation, M. Larrey observes, that "all wounded in such circumstances, on whom the operation is not performed within a few hours after the accident, die on their way to the hospital, or soon after entering it."* But besides the lives lost from delay, it is to be considered that where the work to be done is altogether beyond a man's power to accomplish, there are very few that will not be thrown into hurry and confusion, and do ill what their time allows them to undertake; very few who will have strength of mind in that distress to select what can be done, and apply their thoughts only to what they have decided to do. What will be the conduct of an ordinary man in such a situation? A wound is brought before him, and the life of the patient requires an immediate operation; but a dangerous operation is at all times formidable, and especially on a field of battle, if a surgeon is not within reach of the counsel and asistance of his brethren. Often from timidity, and oftener wearied out by the heart-rending importunities of the poor fellows who are waiting their turn to be dressed, he will be seduced to hope that no harm will happen from a little delay, and to put off the operation.

The only effectual means which we can imagine of supplying relief to the wounded of an army actually in battle, is by such

* Page 486.

an arrangement as M. Larrey has described under the name of *ambulance*. This plan had been suggested by the celebrated Ranby, sergeant-surgeon to George the second whom he accompanied in his German wars. He had witnessed the miserable insufficiency of the ordinary supply for the emergencies of an engaging army, and describes in the little treatise on gun-shot wounds, suggested by his practice in those wars, the method of which he foresaw the success, but which has never been fully acted upon in our service.

"I could wish to be indulged," says Ranby, "in offering a scheme which might, I think, be put in execution, with all the facility imaginable. It is this: when the army is forming for an engagement, let the surgeons, with their respective mates of the three or four regiments that are posted next each other, collect themselves into a body, (the same method being observed throughout the line,) and take their station in the rear, according to the command of the general. Here let the wounded be put under immediate care and management. By this means they will be enabled mutually to assist each other, and to perform their duty both with more exactness and despatch. Now let any person of common humanity reflect on the consequences naturally flowing from this manner of treating the wounded on the spot; having first formed an idea of poor creatures being actually conveying, though with all conceivable tenderness, from place to place, under the extreme misery of large, lacerated wounds, bleeding arteries, or fractured limbs; and this abrupt, preposterous removal attended with perhaps the most fatal symptoms, of which there was not perhaps at first the least appearance or apprehension. I am apt to think that such a one will not hesitate a moment in preferring this my scheme to that scene of terrible distress, which I look upon as one of the most moving that can be presented to the human eye."*

But to make these camp-hospitals effectual, of course another greater alteration in the present system is requisite: a larger medical establishment. When troops are healthy, and not near an enemy, the present establishment of the regiment is more than sufficient; but in an enemy's country, and in an unhealthy

* Ranby on Gun-shot Wounds, page 33.

climate, it is altogether insufficient; and the medical staff of the army is likewise inadequate to the exigencies of the service. The system of warfare is now more bloody than it ever was. The greater use of the field artillery and of the bayonet in our present battles is attended with a havock, which by adding to the number of the wounded, increases the labour of the surgeons, and renders the augmentation of their number absolutely necessary. This augmentation must be adapted to the occasion. When an expedition is fitted out, in which a severe contest with the enemy may be expected, most especially when it is destined to an unhealthy climate, let government show its care of the welfare of its soldiers by raising a numerous but temporary medical staff, as was done in France. It may be done here as easily as in France, and as well. The emulation which Larrey tells us appeared in their schools for the appointment of that service, might be seen in our own, provided government could engage the confidence of the young men of the profession, and satisfy them that merit would find its own way to distinction; a trust which it could easily inspire, as was done there, by its first and leading appointments; nominating to the head and command of the whole medical establishment, not those whose names were presented by the lists of seniority and service, or obtruded by interest, but those whom their own pre-eminence in genius and knowledge had already marked out in the eyes of the country, as the legitimate claimants of the noblest office with which the country can honour the profession—the superintendance of the health and lives of its defenders.

M. Larrey professes to give an historical as well as a chirurgical account of the Egyptian expedition; he is, however, extremely concise, and very properly so, in speaking of whatever relates to the military operations of the French army. The order of their occcurrence has, in a great measure, fixed the order of the professional contents of this volume, and they chiefly serve as dates in the journal which he kept in his own department. As we wish to lay before our readers whatever appears to us most interesting in point of novelty and importance, we shall insert extracts chiefly from the cases, some of which are valuable on many accounts. The chapters on the plague, and on ophthalmia, though treating of the two most re-

markable complaints in Egypt, and obviously the result of much experience, seem to us to contain very little interesting matter: to these therefore, we shall not call the reader's attention; yet we cannot help observing the singular fact that neither M. Larrey nor M. Desgenettes, who have both written on the plague, should have been acquainted with the good effects of mercury in that disease; and yet M. L. had observed the analogy between the plague and the yellow fever of the West Indies, and could scarcely fail to be acquainted with the latter complaint.*

The French lost a number of their wounded from tetanus; in some it proved fatal in three or four days, in a few the disease lasted longer. M. L. observed in his patients, "if not an abhorrence of fluids, at least a considerable repugnance to them;" in consequence of which it was difficult to administer internal remedies. In the case of a surgeon who died of that dreadful affection, in consequence of a wound of the face, he endeavoured to introduce into the œsophagus a tube of elastic gum; but he found it impracticable, as it occasioned convulsions and suffocation. On opening the bodies of patients that had died of locked jaw, he found "the pharynx and œsophagus in a state of great constriction, their internal membrane red, inflamed, and lined with a viscid and reddish substance."†

Frictions with oil, strongly recommended by some authors, were inefficacious. Mercurial frictions seemed to M. L. to aggravate the symptoms. He tried, but without any success, an application of tobacco leaves, to the wounds of patients affected with tetanus. Moxa and the actual cautery were equally unsuccessful. He was not more fortunate from the use of blisters, except when the tetanus was preceded by the cessation of suppuration from the surface of a wound; in such cases he applied blisters with advantage, and their good effects were attended with a renewal of the suppuration. The favourable result of a case of tetanus in an officer, who was shot in the foot, and whose leg was amputated, after opium, tobacco, camphor and

* The same analogy has been observed by Drs. M'Gregor and Chisholm.

† Page 58.

other remedies had been used in vain, led M. Larrey to recommend the same practice in other cases; and his success supports the opinion which he advances, that in all cases of tetanus from wounds of the limbs, it would be adviseable to amputate as soon as the symptoms appear. When the officer whose case we allude to submitted to amputation, his life was despaired of; he himself wished for the operation, and it was performed as a last resource, after every other remedy had been tried in vain. The symptoms ceased almost immediately on removing the limb in which the wound was situated; even where the case in the end terminated fatally, the operation relieved the symptoms very considerably. It should seem however, from one case, that a division of the soft parts in the vicinity of the wound, was sufficient to relieve the symptoms of tetanus, at least in the beginning. The general of division, Destaing, was shot through the middle and back part of the right arm, at the battle of Alexandria; the ball had penetrated through the muscles and had injured the nerves of the arm; convulsive motions of the limb, considerable anguish, and some difficulty of breathing took place soon after the wound was inflicted. At the end of eight days, during which the patient had suffered considerably from pain, febrile symptoms, and loss of appetite, M. Larrey was called in. He wished immediately to lay open the wound along the course of the ball, but to this the patient refused to submit, and emollient applications were employed. However, on the following day, symptoms of tetanus having made their appearance, and the patient being in a state of great anxiety, the wound was laid open by dividing the integuments, the cellular membrane and the few muscular fibres which covered the course of the ball; the lower part of the wound was likewise scarified. The operation was painful, but in two hours the patient felt considerably relieved, and at the end of two days the alarming symptoms entirely disappeared, and the patient recovered.*

During the campaign in Egypt, the operation of extirpating the arm at the shoulder joint occurred nineteen times; thirteen of the patients did well, the remaining six died of internal diseases.

* Page 81.

This operation has been performed several times in this country, and by our army surgeons; and the manner of performing it is so generally known, that we deem it unnecessary to say any thing further on that subject.

We do not know that the operation of extirpating the thigh at the hip joint was ever performed by any surgeon in this country except by Dr. Kerr of Northampton.† M. Larrey has performed it three times, twice in Egypt and once while he was surgeon to the army on the Rhine. His three patients died; but when it is considered that one of these survived the operation a week, at the end of which he was carried off by the plague, and that the others died after being conveyed in a very uneasy manner during a precipitate march of the army, we are not warranted, especially after the successful case that is already upon record,‡ in declaring the operation to be impracticable, and we should think very ill of a surgeon who, under the circumstances in which it is called for, should not give his patient the remaining chance of life.

The excision of joints is one of the most important improvements of modern surgery, and for the manner in which M. Larrey has employed that practice in the treatment of gun-shot wounds, he deserves the greatest credit. He has been able to save limbs, which according to the rules of surgery, should have been taken off; and when the importance of saving an upper extremity, for which no substitute can be supplied, is considered, the value of his practice may be appreciated. When the arm is fractured near its upper extremity by a musket ball, it is considered by most surgeons necessary to amputate the limb; and in such cases the operation used invariably to be performed; but says M. Larrey,

" I have had the good fortune, on ten different occasions, of superseding the necessity of the operation, by *the complete and immediate extraction of the head of the humerus and of the splinters.* I perform the operation in the following manner. I make an incision in the centre of the deltoid muscle, and parallel to its fibres, carrying the incision as low down as possible.

† Vide Dr. Duncan's Med. Commentaries, vol. vi. p. 337.

‡ Sabatier Medicine Operat. tom. iii. p. 330.

I get the edges of the wound drawn asunder, in order to lay bare the articulation, of which the capsule is generally opened by the first incision, and by means of a probe pointed bistoury I detach with the greatest ease from their insertions the tendons of the supra and infra spinati, of the teres minor, of the infra scapular, and of the long head of the biceps; then I disengage the head of the humerus, and remove it through the wound in the deltoid by means of my fingers, or of an elevator. I bring the humerus up to the shoulder, and fix it in a proper position by means of a sling and a bandage. Such is the operation which I performed on ten patients, in extirpating the head of the humerus; one of these died of the hospital fever, two of the scurvy, at Alexandria, and the fourth, after he was cured, died of the plague in our return to Syria. The rest returned to France in good health. The arm became anchylosed to the shoulder in some, and an artificial joint allowing of motion was formed in others."*

Our limits forbid our giving the details of the cases in which this operation was performed, some of which are highly interesting. In the case of a drummer, seventeen years of age, whose shoulder was struck by a four-pounder, the humeral extremity of the clavicle, the acromion and the head of the humerus were so shattered that it was necessary to remove all these parts; this young man recovered completely, and his arm became anchylosed to his shoulder.

Perhaps the most important part of M. Larrey's work, is that on amputation in consequence of gun-shot wounds; this memoir as it is called, was written as an inaugural dissertation for the degree of doctor of medicine, and its principal object is to recommend immediate amputation in all cases of gun-shot wounds which require that operation. In the year 1754, the academy of surgery offered a prize for the best dissertation on gun-shot wounds, and on the most proper time for performing amputation, when that operation became necessary in consequence of such wounds. After considerable debates, the prize was adjudged to the dissertation of M. Faure. The object of his dissertation is to recommend a delay of the operation, and

* Page315.

M. Larrey observes, that the principles of that author have been since adopted by almost all practitioners. The practice of M. Faure is followed by the most eminent surgeons of the present day in this country; it is recommended by Mr. John Hunter, in his treatise on gun-shot wounds; the support however which it derives from the notice of Mr. Hunter, arises more from the authority of his name than from the strength of his arguments in its favour. That part of Mr. Hunter's works is in our opinion, the weakest of all his practical writings. The propriety of immediate amputation, when its necessity is admitted to be sooner or later unavoidable, is enforced by writers whose experience gives considerable importance to their opinions. Suffice it to name Le Dran, Ranby, Kirkland, and Larrey. It would be presumptuous in us to attempt to decide a question so important and so difficult; but after considering the subject with attention, we believe it may be safely affirmed that the propriety of delaying amputation till the first inflammation is over, is far from being so firmly established as Mr. Hunter thought, and as many surgeons still maintain. The work of M. Larrey contains a collection of facts more conclusive in favour of immediate amputation than any that we have met with in any other author; and he reasons from the only experience to be relied on, viz. an experience of the comparative merits of both methods. It would exceed our limits were we to attempt to lay before the reader an analysis of that part of the work; we must content ourselves with having given our opinion of its importance, and refer to the work itself for the details.

Among the most singular wounds that occurred in M. Larrey's practice is one of a soldier, whose epiglottis was shot off at the battle of Alexandria. "The ball entered at the angle of the jaw, crossed the mouth obliquely, and went out at the opposite side, (*et sortit à la region jugulaire du côté opposé.*) The base of the tongue was grazed and the epiglottis shot away; the patient spat it out some time after, and showed it to the surgeon who first dressed him. The patient suffered little, but he could not produce articulate sounds, and his voice, which was hoarse and feeble, was heard with difficulty." Every attempt which he made to drink was attended with a convulsive cough, and with a sense of suffocation and vomiting; he

remained in that deplorable condition for four days; on the fifth day M. Larrey was called in; h introduced into the œsophagus a flexible tube, through which the patient was fed for six weeks; at the end of that time he was able to eat without the tube thick panado and thickened rice, which he formed into little balls, and swallowed with tolerable ease. His powers of speech and deglutition improved very much afterwards, in consequence as M. Larrey imagines, of the office of the epiglottis being supplied by an enlargement of the arytenöid cartilages, and by an expansion of that part of the base of the tongue which lies next to the glottis.*

Another very singular circumstance observed by M. Larrey in Egypt, was an atrophy of the testicles, from which a number of the French soldiers suffered.

"Both testicles, but more generally one, loses in part its sensibility, becomes soft, diminishes in size gradually, and appears to dry up. The patient is not sensible of his loss till the testicles are very much reduced in size, and then they are found lying at the ring, of the shape and size of a French bean. All those to whom this accident has happened, have lost all venereal desires, and become impotent, and the whole system has been weakened in consequence of that loss."†

We have already exceeded our proper limits, and yet our analysis is very imperfect; but we trust the reader will be able from these extracts to form some opinion of the merits of this volume. It is the work of one of the first military surgeons of the present day, raised to high rank in his profession at the beginning of a revolution, a time when merit never fails to be discovered and rewarded. To know that he has been employed in the principal campaigns of the French armies, is sufficient to give an idea of the extent of his experience. On his return to France from Egypt, he was appointed surgeon to the consular guard, and we understand that he is now body surgeon to Bonaparte. We strongly recommend this volume to the perusal of all surgeons, but particularly to army surgeons: it is to be regretted however, that copies of it cannot be readily procured in this country, and that no translation of it has yet been published.

* Page 290. † Page 216.

The Anatomy and Surgical Treatment of Inguinal and Congenital Hernia. By ASTLEY COOPER, F.R.S. &c. &c. Illustrated by plates. London, 1804. Large folio. 2*l.* 2*s.*

[From the Edinburgh Medical and Surgical Journal for 1806.]

THIS work is already so well known that some of our readers may think it necessary at this time to take notice of it; but the merits of the author, and the importance of the subject, induce us to give a short analysis, and to make a few cursory remarks on its contents.

The object of Mr. Cooper, in the present volume, is to give the anatomical description and surgical treatment of inguinal and congenital hernia, leaving the consideration of femoral, and the other species, to a future publication.

There is no disease where minute anatomical dissection has been more successfully employed than in the pathology and treatment of hernia; for as this complaint arises more from an alteration in the relative situation of organs than from any morbid change of structure, we must be well acquainted with the natural appearances in order to discriminate the effects of disease, or be able to point out the best means of affording relief.

Although Albinus seems to have been well acquainted with this part of anatomy, yet it is to Gimbernat, a celebrated Spanish surgeon, that we are indebted for the first accurate and detailed description of the *abdominal*, and still more particularly of the *crural* rings.

Mr. Cooper, besides availing himself of the description these anatomists have given, advances a step further, and describes some of the more minute parts of the anatomy of the abdominal ring with much precision and ingenuity. Neither Mr. Cooper, however, nor, as far as we know, any other anatomist, has paid sufficient attention to a description of the bones of this part of the pelvis, when describing the soft parts; for we have long believed that an accurate knowledge of them will lead to a simple description, and give a clear notion of the soft parts with which they are connected. If we examine these bones, we find a ridge extending from the inferior part of the

venter of the ilium, which forms the lateral parts of the brim of the pelvis, and the line of division between the pelvis and abdomen, called *linea innominata;* and there is also another line or ridge, which begins at the rough crest or angle of the os pubis, and extends along the upper and inner edge of the bone to meet the former. The junction of the two ridges, in some subjects, is distinctly marked; but, in most bones, a small portion of the edge is rounded at that part where the femoral vessels pass out of the pelvis. Although these two ridges have been described long ago by anatomists, yet they do not seem to have explained their use, or considered them in the description of the soft parts. It will be found, however, that the pubic portion is intended for the insertion of the inflected part of the ligament of Poupart, called Gimbernat's ligament, and the iliac portion for the attachment of the aponeurosis of the iliac, psoic, and abdominal muscles. This knowledge of the bones, and of the different parts of them into which the aponeurotic and tendinous fibres are inserted, enables us to form a clear and distinct notion of the relative situation of parts, and to be aware of the rashness with which particular names have been given to continuations or parts of one fascia, or of the same aponeurotic expansion.

The oblique passage of the spermatic cord through the abdominal parietes was well known to and elegantly delineated by Albinus. Nevertheless, no author, except Mr. Cooper, has considered it with that attention which it seems to merit. After having minutely described this beautiful piece of mechanism, he points out, with a good deal of ingenuity, the necessity of taking it into consideration, in order to explain particular symptoms and varieties of inguinal hernia, to accomplish the reduction of hernia, and also in the application of trusses.

There is one part of the anatomy of which our author has not taken notice. It is the difference of the relative situation of the ligament of Poupart in the male and female. In Mr. Cooper's description no other difference is mentioned, than that the abdominal ring serves for the passage of the spermatic cord in the male, and of the round ligament in the female. Dr. Monro, jun. has observed a remarkable difference in the

formation of these parts in the two sexes, and he has described it accurately, and delineated it in his work on femoral hernia. It may be sufficient here to mention, that this difference accounts very satisfactorily for the greater frequency of femoral hernia in the female than in the male, and for the rare occurrence of inguinal hernia in the female.

The next part of Mr. Cooper's work treats of the pathology of hernia. He describes the manner in which the sac is formed, its size, mode of descent, coverings of fascia, cellular substance, and, as he calls it, cremaster muscle. He also takes particular notice of the situation of the spermatic cord, and mentions two varieties, in one of which, instead of the hernial sac passing on the outside of these vessels, it was found on their inner or pubic side. In the other instance, the vas deferens was found on the one side, and the rest of the cord on the other side of the sac.

The next chapter contains the diagnosis and the enumeration of those causes which most commonly produce hernia.

The subject of the fifth chapter is one of importance, and it is full of good practical information relative to the mode of action, application, and management of trusses.

"The object in applying a truss (says our author) is to close the mouth of the hernial sac and destroy its communication with the abdomen; and this object can never be perfectly fulfilled by any truss which is applied in the usual manner upon the abdomnial ring, and extending from it upon the os pubis. In this case the cure must be incomplete, because a considerable portion of the hernial sac remains uncompressed towards the abdomen, which portion is that situated between the abdominal ring and the opening of the sac into the cavity of the belly." He adds, "The proper method of completely obliterating the mouth of the hernial sac is to apply the truss, not upon the abdominal ring, but upon the part at which the spermatic cord, and with it the hernia, first quit the abdomen; for in this way only can a descent of the hernia be prevented entirely, and a cure by pressure, if practicable, can be performed." "Therefore when a hernia has been returned into the abdomen by the surgeon, he should lay his fingers obliquely above and without the ring, and direct his patient to cough, and the furthest part

from the ring towards the spine of the ilium, where the hernial sac is felt to protrude, is the point which should be noted for the application of the pad of the truss, and the instrument made accordingly."

These remarks are no less ingenious than important. There are two circumstances, however, which appear to us to be of some moment, although they have not been taken notice of by our author. They regard the strength of the spring, and the form and size of the pad. It will be found that a person who has worn a truss for many years is not able to give up using it, chiefly on account of the strength of the abdominal muscles being diminished at that part where the pad rests. The pressure of the pad seems to produce an absorption of the abdominal parietes. As this effect will always be in proportion to the degree of pressure or strength of the spring, it is of importance never to employ in any case a spring stronger than what is necessary to keep up the bowels; and it is useful for a person labouring under hernia to have two trusses of different strengths, one for ordinary purposes, and the other when riding, or when taking any violent exercise. Our author advises that the pad be made of a conical form, the apex of which should rest on the internal ring or mouth of the sac. However useful this may be as a general rule, it ought to be known that there are many exceptions; and, with due deference to Mr. Cooper's opinion, we beg leave to mention, that we have found such a variety, not only in the size of the rupture, but also in the situation and size of the opening through which it passes, that we apprehend it will be necessary to vary the form and bulk of the pad according to every individual case. If the pad is too large and flat, it will prevent the bowels from passing through the external ring, but it will allow them to pass through the internal ring, and enter the inguinal canal. On the other hand, if the pad is so small as to press into the mouth of the sac, and plug it up, there is no longer any chance of a permanent cure. The bowels may be prevented from entering the sac, but the pad will act as a dilator or bougie, keep the mouth of the sac continually open, and even increase its diameter. It therefore appears to us, that, in every case of reducible hernia,

a pad ought to be made according to the size and situation of the ring; that it be of such bulk and form as to make a pressure on the *internal* abdominal ring, along the *inguinal* canal, and on the *external* ring.

IRREDUCIBLE HERNIA.

"The following are the cases which seem to bring the disease into this state.

1. When the protruded parts are suffered to remain long down, they increase so much in size as to be incapable of reduction.

2. When membranous bands form across the sac, entangle its contents, and prevent its free motion.

3. When the protruded parts become closely united by an adhesion to the side of the sac sufficiently firm to render them immovable."

The most efficacious means recommended by our author to accomplish the reduction of a hernia in this state, is to wear a bag truss of such a form as to keep a steady and uniform pressure on the scrotum. The pressure produces a gradual absorption of the adipose matter of the protruded bowels, and thus, after some days confinement, the tumor becomes very much diminished, and at last may be returned. The application of ice occasionally procures the return of a hernia which appears irreducible. Mr. Cooper thinks that the good effects of this remedy are owing to its producing a contraction of the scrotum, which performs the office of a strong and permanent compression of the tumor, and he relates two cases where this practice proved successful.

OF STRANGULATED HERNIA.

"If the tumor be examined after death, a quantity of clear serum will first be found under the skin. The hernial sac contains a portion of bloody serum of a coffee colour. The intestine is of a chocolate brown, with here and there a black spot, which easily breaks down on being touched with the finger. A coat of the coagulable lymph, of the same colour as the intestine, may be peeled from its surface, and adhesions of no great strength are found to extend from the intestine to the sac. At

the particular part where the intestine is strangulated by the constricting membrane, it is either ulcerated through, or readily pulls asunder under slight pressure. If the inflammation has been very extensive, there is a quantity of air in the surrounding cellular membrane."

"On examination after death, in strangulated omental hernia, the omentum is found scarcely changed from its natural appearance; its colour is a little, and but a little, darker than usual. I found it in some cases, even during the operation, extremely offensive to the smell; there is scarcely any fluid in the sac. Though the cavity of the abdomen is inflamed, and the intestines slightly adhering to each other, they never appear to have suffered so much as by intestinal hernia." In old and large herniæ, Mr. Cooper believes that the strangulation is formed most frequently by the external abdominal ring; but, in other cases, it is more commonly seated at the internal ring, the place where the spermatic vessels quit the abdomen. "So if the surgeon, during the operation for hernia, examines accurately into the seat of the strangulation, he will find, except in large herniæ, cutting through the ring is insufficient to relieve the protruded parts, but he must proceed with his knife further up towards the spinous process of the ilium, before he can return the swelling."

"Moreover, though the abdominal ring be dilated with freedom, the hernia will, in many cases, still retain its colour of strangulation, and remain irreducible, as before; but if the sac be traced upwards with the knife, about one inch and a half, midway between the ilium and pubis, the stricture will there be found; and, when this is divided, the intestine recovers its colour, and can be readily returned."

TREATMENT OF STRANGULATED HERNIA.

The first object of our attention, in order to accomplish the reduction of a strangulated hernia, ought to be the position of the body of the patient. The celebrated Winslow, the first anatomist who explained many of the phenomena of the disease from a previous knowledge of the action of the muscles, conceived it to be of importance that the body of the patient be placed in an inclined plane, and that the thighs be bent towards

the trunk of the body. Our author advises the same practice, and remarks that such a posture, by relaxing the fascia of the thigh, relaxes also the aperture through which the hernia passes. There is no doubt but every degree of tightening or relaxation of the femoral fascia will be accompanied by a corresponding change in the abdominal ring; but the motion of flexion, while it relaxes the fascia, relaxes at the same time the abdominal internal iliac and psoi muscles, and it is the change produced by the relaxation of these muscles which facilitates, and ought to be kept in view in the reduction of hernia. To be convinced of the truth of this observation, it is only necessary to observe in the dead body, when in a horizontal posture, the size of the crural ring. By introducing into it the forefinger, a tight cord is readily felt at the upper part; when the thigh is bent upwards and inwards, the cord is relaxed, and the opening is enlarged.

The pressure which is employed on the tumor by the hands of the surgeon should always be directed upwards and outwards along the course of the canal of the cord, and our author advises it to be continued from a quarter to half an hour; besides the mechanical means, he recommends tobacco glisters and cold as the most successful in diminishing the increased action and bulk of the parts. Cold applications have been approved of by the most celebrated surgeons, and they have been particularly recommended by Mr. B. Bell, in the form of ice: when ice cannot be procured, our author uses a mixture of equal parts of nitre and sal-ammonia. To one pound of water in a bladder ten ounces of the mixed salts are added, the bladder tied up, and then laid over the tumor. "If, after four hours, the symptoms become mitigated, and the tumor lessens, this remedy may be persevered in for some time longer; but if they continue with unabated violence, and the tumor resist every attempt to reduction, no further trial should be made of the application."

OPERATION.

There is not a more difficult point in surgery, or one which requires more decision, than to determine the exact period when recourse must be had to the operation. From the dread-

ful consequences of delaying it till the protruded parts mortify, some eminent surgeons have recommended its early performance, whilst others, from the severity and risk which always attend it, advise it to be delayed till every means have failed of procuring a reduction.

Those who urge the early performance of the operation (more particularly Dessault) have founded their opinion on the effects of a similar operation, when there is no hernia, or when the hernia is recent; for, in such cases, no serious consequences are to be apprehended, or any symptoms likely to occur more violent than what takes place after the common operation of hydrocele by incision; on the other hand, many cases have occurred, where, after repeated trials had been made to accomplish a reduction, and the operation about to be performed, the bowel has been suddenly and unexpectedly reduced.

The symptoms which ought to guide us in having recourse to the operation arise from an attack of inflammation in that part of the intestine contained in the hernial sac, and from its spreading into the abdominal cavity. It is in proportion to their violence, and after every fair and probable means has been employed, that we ought to urge the performance of the operation. Mr. Cooper considers pain on pressing the belly, and tension, as the symptoms which point out its immediate necessity. He adds, page 27, "Indeed, there is scarcely any period of the symptoms which should forbid the operation; for even if mortification has actually begun, the operation may be the means of saving life, by promoting the ready separation of gangrenous parts."

Mr. Cooper has explained at great length the different steps of the operation. He directs the incision to be made from the upper part of the abdominal ring to the bottom of the hernial sac. We are warned, however, by Camper against making this extent of incision downwards, for it sometimes happens that the spermatic vessels pass on the anterior part of the sac, and are very apt to be divided.

The sac is to be opened at its inferior part by pinching it up with a pair of forceps, and cutting the elevated portion horizontally with a scalpel.

We witnessed, in one case, a surgeon very much perplexed

when he came to this step of the operation. The sac had a blue transparent colour, and looked very like a piece of strangulated intestine; the surgeon, for some time, conceived that it actually was so, and at last opened it with the utmost terror, when, to his surprise, he found it to be a thin hernial sac, much distended with water. We have seen the same puzzling appearance in the operation for hydrocele by incision, and it is one we may expect to find when either the hernial sac or tunica vaginalis is thin.

In order to divide the stricture, "the surgeon passes his finger into the sac as far as the stricture, and then conveys a probe-pointed bistoury on the fore part of the sac, and, insinuating it within the ring, cuts through it in a direction upwards, opposite to the middle of the sac."

Mr. Cooper thinks there is an advantage from not dividing the *hernial sac* in dilating the ring, as it takes away all danger from wounding the intestine; he also makes some useful remarks on cases where there was more than one stricture.

OF MORTIFICATION OF THE BOWELS.

The next chapter contains many useful practical observations on the mortification of the bowels, and on the artificial anus; also some very interesting and ingenious experiments of Mr. Thomson of Edinburgh, relative to the mode of tying two portions of divided intestine.

It appears from Mr. Thomson's experiments, that if the intestine of an animal be divided transversely, reunited by ligatures, and returned into the abdomen, the animal suffers no inconvenience, and the ligatures are discharged at the anus.

"However (says Mr. Cooper,) as the protruded parts in hernia are so much inflamed as to endanger a speedy separation of the ligatures, and as it appears from my experiments (page 35) that the animal did not suffer from the ligature hanging from the abdomen, I should still prefer performing the operation of uniting the divided intestine in such a manner as to give an opportunity of extracting the ligatures, if any inconveniences arose from their application."

Since Mr. Cooper's work appeared, an essay has been published at Philadelphia by Mr. Smith, which contains a series

of ingenious experiments on the wounds of intestines. The conclusions he has drawn we shall quote in his own words.

"It appears from the result of my experiments on dogs, that not only the intestine may be returned into the cavity of the abdomen, but that the ligatures may be cut off and returned with the intestine, as was observed by Mr. Thomson of Edinburgh, and that we need not be under any apprehension of their being discharged into the cavity; for, by some process of the animal economy of which we are ignorant, the ligatures have, in every instance, either been discharged with the fæces, or been found loosely attached to the *internal* coat of the intestine.

"It has been said by Messrs. Cooper and Thomson, that there is a curious difference in the facility with which a longitudinal and transverse wound unites. But, in all the experiments which I have made, it was found that, with care, the *longitudinal* united as kindly as the transverse, only requiring a little more attention to the diet of the animal, which should be very sparing and liquid, until the wound has had time to heal. It certainly requires more pains to close a longitudinal wound completely than one which is transverse. The longitudinal incision always occasions a diminution in the diameter of the intestinal canal, thereby producing dangerous obstructions. If it be of any considerable extent, probably the surgeon would be justified in cutting out the wounded portion, and treating it as a transverse division. This may be done without much endangering the life of the animal, as appears by two experiments, where three inches of the intestine were removed."*

Mr. Cooper next proceeds to give some account of the mode of dressing, and after-treatment of the patient. He particularly takes notice of the impropriety of giving strong purges, if an evacuation can be procured by more gentle means. He enjoins the patient to be kept in a horizontal posture till

* Vide an Inaugural Essay on wounds of the intestines, for the degree of doctor of medicine, submitted to the examination of the Rev. J Andrews, D. D Provost (pro tempore,) the Trustees and Medical Professors of the University of Pennsylvania, June 1805. By Thomas Smith, of the island of St. Croix.

the cure is complete, and shows the necessity of wearing a truss during the rest of his life.

The object of some writers to produce a radical cure by tying the mouth of the sac, Mr. C. considers as ineffectual and dangerous. Although the opening of the peritoneum is shut up by such an operation, the dilatation of the abdominal ring must ever remain open.

OF LARGE HERNIA.

Under this head our author records two important cases of large hernia, in one of which the bowels were reduced, after dividing the stricture, without opening the sac; in the other the sac was opened. The first patient recovered, the second died.

These cases, and the conclusions which our ingenious author has drawn from them, present to practical surgeons a point worthy of their mature consideration, Whether ought we, in general, to open the hernial sac? This mode of operation was first proposed by Dr. Monro, sen. and several cases are related in the appendix to his work on the bursæ mucosæ, where it was performed most successfully.

OF SMALL HERNIA.

Our author remarks, that it is by no means unfrequent to meet with cases of hernia where the hernial sac is so small as not to extend through the abdominal ring; and as in such there is little appearance of external tumor, the disease is very apt to be overlooked by the patient and surgeon, and some other cause assigned for the series of symptoms. The manner of operating in this form of the disease differs from that in the common scrotal hernia: the incision is to be made parallel to the direction of the spermatic cord, and the stricture will be found at the internal ring.

OF INGUINAL HERNIA ON THE INNER SIDE OF THE EPIGASTRIC ARTERY.

This variety of hernia, says Mr. C. has now very often fallen under observation, and has been for more than 25 years

described in the lectures delivered at St. Thomas's and Guy's Hospitals.

The following note, which is translated from Rougemont, the author of the French translation of Richter's treatise on hernia, will also show that this form of the disease has not only been *long* known and *well* understood on the continent, but the same mode of operation as Mr. Cooper mentions has been adopted.

"After all these considerations, I conclude with Mr. Dessault, that the epigastric artery in inguinal hernia is commonly placed near the *internal* angle of the ring, and rarely towards the *external*. The cases where that artery is placed at the external angle of the ring in inguinal hernia are very rare, and they do not happen unless when the viscera escape on the inner part of the ring, and then the cord is placed to one side, and a little behind the sac. I have had occasion, two years ago, to observe this distribution on a dead body; I preserved the preparation for several months, and showed it to several professional men."

Afterwards he adds,

"I believe, after what has been said, that we may be allowed to conclude that we run less risk of wounding the epigastric artery in cutting upwards and outwards, than in cutting upwards and inwards; that, in order to know exactly the situation of that artery, we ought to become acquainted with the relative situation of the spermatic cord and hernial sac; and, supposing that this is impossible, *we ought to make the incision directly upwards, through the middle of the superior edge of the ring.*"

OF CONGENITAL HERNIA.

The author, under this head, has nothing particularly interesting to communicate, except a case described by Mr. Foster, where, on dissecting carefully through the tunica vaginalis of the cord near the ring, a fluid escaped. "I then (says Mr. F.) continued the incision to the bottom of the scrotum through the tunica vaginalis of the cord, and the tunica vaginalis testis, which I found to be one cavity, the edges of which being turned back on each side exposed a hernial sac pendent from the ring, and descending towards the testicle." Mr. Cooper adds, "The idea I have formed of this case is,

that the tunica vaginalis, after the descent of the testis, became closed opposite to the abdominal ring, but remained open above and below it. That the intestines descended into the upper part, and elongated both the adhesion and tunica vaginalis, so as to form it into a bag, which descended into the tunica vaginalis below the adhesion, and becoming wide at its neck, though narrow at its fundus, it received a portion of intestine, which was too large either to be returned into the abdomen, or to retain its functions, whilst it continued in the sac."

The work is embellished with eleven plates, illustrative of the anatomy and pathology of hernia. These and the letter-press are in very large folio, and although this size adds to the splendor of the book, it renders it so importable, and so teazing and difficult to read, that we cannot help thinking, that if the same materials had been printed in a different form, and sold at a more moderate price, it would have been more generally useful, and more within the reach of the great body of readers.

As drawings, we cannot help regretting that, situated as Mr. Cooper is in the focus of the arts, he had not been fortunate enough in employing a more able draughtsman. To us they appear to be executed with great stiffness, formality, and labour; they seem not as they were drawings from the subject, but as copied from heavy brass casts.

When we compare them with Camper's drawings on the same subject, our English artist is completely eclipsed. Camper, with all the spirit and power of a master, expresses, with a single well chosen line, what Kirtland tries to do by a hundred.

As engravings, they are well executed, but they are loaded and heavy with much unnecessary labour and superfluous work. They are far from being the *chef-d'œuvres* of Heath, and seem to be indebted to him for nothing but his name.

These remarks, however, apply only to the plates as works of art; with regard to their anatomical accuracy, we believe them to be true representations, and certainly they will ever be considered as a useful addition to surgical anatomy.

From this outline we have given of Mr. Cooper's work, we hope our readers will be enabled to form a general idea of its contents, and to appreciate its merits. The attentive practi-

tioner will, we venture to say, find in it much useful practical information; and the opinions and scattered observations of former authors, and of illustrious teachers, are so well arranged and so judiciously collected, as sufficiently to recommend the work to the careful perusal of every description of medical men.

The Anatomy and Surgical Treatment of Crural and Umbilical Hernia, &c. &c. By ASTLEY COOPER, F. R. S. *Honorary Member of the Medical and Physical Societies of Edinburgh, Lecturer on Anatomy and Surgery at Guy's Hospital. Illustrated by Plates.* Part II. 90 pages, atlas folio. London, 1807.

[From the Edinburgh Medical and Surgical Journal for April 1808.]

In our Journal for April 1806, we took an opportunity of giving a general view, and short analysis of the first part of Mr. A. Cooper's work on Hernia; and we now have the satisfaction of performing the same duty to the second part, which fulfils his promise, and completes his work on this interesting subject. In this volume Mr. Cooper comprehends the anatomy and treatment of the crural hernia, of the umbilical, ventral, pudendal, perineal, thyroideal, cystic, phrenic, mesenteric, and mesocolic herniæ, and of the strangulated intestine.

We formerly remarked the success of Mr. Cooper's investigations in the anatomy of the *inguinal ring*, and he seems to us to have been equally diligent, and no less successful in the dissections which he has now given of the *crural ring*. The complicated structure, however, of this part of the body, but, above all, the imperfection in the language of anatomy which is generally adopted, renders it extremely difficult to describe these parts with clearness and precision. Indeed, in no part of anatomy could the necessity of adopting the ingenious and precise language of Dr. Barclay be more strongly urged; for, notwithstanding the patient investigation Mr. Cooper has bestowed, his description is not altogether perspicuous, and it is with difficulty understood, unless by those who have the parts lying before them, or who are very familiar with dissection. From an aim at too great minuteness, we are convinced that the

structure of these parts has been made to appear much more complicated than it really is; and the insatiable desire which several anatomists of the present day have shown to multiply fasciæ, has at last been carried so far, and indulged to such a degree, that it appears as if no one believed himself entitled to any claim of excellence, unless he has added one to the list; just as many young surgeons conceive themselves incapable to perform an operation until they have contrived some new instrument: although it may in general be remarked, that the best practical surgeons invent fewest instruments, and the most experienced anatomists make the fewest discoveries.

In the anatomical part, Mr. Cooper first gives a description of the bones, then of the soft parts of the thigh which are adjacent to the cavity of the abdomen, and afterwards of those which shut up the abdomen from the thigh.

He describes one fascia, called the *superficial* fascia, which lies immediately underneath the skin and fat, extends over the tendon of the external oblique muscle, adheres firmly to the crural arch, passes downwards upon the absorbent glands of the groin, and extends down the thigh. Another fascia, called the *fascia lata*, lies underneath the former, and has two origins, one from the rounded edge of the crural arch, along the space between the tuberosity of the pubes and the anterior spinous process of the ileum; the other from the ligament of the pubes at the insertion of the external oblique muscle, extends over the pectineus and triceps muscles, and having united with the first mentioned part, under the saphena major vein, the united portions extend over the thigh, embracing the muscles, and forming an external covering to the femoral vessels as they emerge from the abdomen.*

The abdomen is described to be shut up from the thigh by two fasciæ, the *fascia transversalis* and the *fascia iliaca*. These, however, ought properly to be considered as only one expansion, which covering the transversalis, the psoi, and iliac muscles, and lying underneath the femoral vessels, passes down

* These two portions of fascia, when united, do not form one plane surface, but give a covering to the hollow space formed by the pectineus and the tendons of the psoi and iliac muscles, as may be seen accurately represented by Mr. Cooper in his former volume.

in the shape of a funnel through the crural arch, and forms the *crural sheath*. The space called the *crural ring* is also within this sheath, and is situated between the crural vein and the crescent-shaped edge of the insertion of the external oblique muscle into the pubes.

Besides passing down to form the crural sheath, the fascia which lines the muscles of the belly, or that portion called *fascia transversalis*, is also said, by Mr. Cooper, to extend over the crural ring. In the dissections we have made, we have never been able to demonstrate such a fascia. The crural ring, in most people, is filled up by an absorbent gland, or by a number of absorbent vessels and fat, so that, in these, no such fascia can exist. Where there are no absorbent vessels or glands, the crural ring is covered by a continuation of the cellular stratum which lies between the peritoneum and the fascia lining the abdomen. If the finger is pressed upon the crural ring, it may be passed from one half to three fourths of an inch towards the thigh within the sheath; and there is no aperture in this part of the sheath, except the minute cribriform holes for the passage of the absorbent vessels, or a single hole if they pass in a cluster. When the finger is thrust down through the crural ring, the lunated or semilunar edge of the fascia lata may be distinctly felt.

Crural Hernia.—After accurately describing the symptoms remarked by the patient, Mr. Cooper mentions that this hernia is generally found to be a small circumscribed tumor, about the size of the last joint of a finger, situated under the crural arch, about an inch on the outside of the tuberosity of the pubes, and lying in the hollow between this process and the crural artery and vein.

"As the tumor enlarges, instead of falling downwards like the inguinal hernia, it passes forwards, and often turns over the anterior edge of the crural arch, this being the direction in which there is the least resistance. As it proceeds, the swelling increases more laterally than upwards or downwards, so that it assumes an oblong shape, the longest diameter being in a transverse or horizontal direction.

"In the female it is generally very movable, and being soft, and the skin not being discoloured, it has the appearance merely of an inguinal tumor of one of the absorbent glands;

but, in the male, the skin is generally not so loose, the swelling not so distinctly circumscribed, and the tumor appears buried more in the substance of the thigh.

"The direction in which the crural hernia passes is obliquely inwards and forwards, and excepting at first, very little downwards, so that, in cutting into this tumor, the incision is made into its fundus. This is the general situation of the tumor, but it sometimes happens, that instead of crossing the thigh in the direction of the crural arch, it extends downwards along the edge of the crural vein and the vena saphena major.

"The crural hernia, when dissected, presents the following appearances: when the skin is removed, the superficial fascia of the external oblique muscle is laid bare, which, though it is of a delicate texture in its common state, when pressed upon by a hernia, becomes extremely thickened, and very distinct, more especially in a subject loaded with fat. Under this covering there is generally another fascia, precisely of the form of the hernia itself, and which it very closely embraces. A thin fascia naturally covers the opening through which the hernia passes, and descends on the posterior part of the pubis. When the hernia, therefore, enters the sheath, it pushes this fascia before it, so that the sac may be perfectly drawn from its inner side, and the fascia which covers it left distinct. The fascia which forms the crural sheath, and in which are placed the hole or holes for the absorbent vessels, is also protruded forwards, and is united with the other, so that the two become thus consolidated into one. If a large hernia is examined, this fascia is only found to proceed upwards, as far as the edge of the orifice on the inner side of the crural sheath by which the hernia descends, but, in a small hernia, it passes into the abdomen as far as the peritoneum, and forms a pouch, from which the hernial sac may be withdrawn, leaving this forming a complete bag over the hernia.

"In a small hernia, the fascia is thicker than the sac itself, but, by being gradually extended, it becomes thinner and less distinct; and, in one example of this kind from the female subject (which forms the subject of one of my plates), this and the superficial fascia have coalesced into one. I first observed this fascia in dissecting a male subject brought into St. Thomas's

Hospital in the year 1800, who had a strangulated crural hernia on the one side, and a reducible one on the other. I next saw it in the operation performed upon Mrs. Bispham, hereafter to be mentioned, and have ever since demonstrated it in preparations whilst delivering my lectures on crural hernia. It may be termed the *fascia propria* of the crural hernia. When this fascia is divided, a quantity of adipose membrane is found between it and the sac, and when this is cut through, the peritoneal sac itself is exposed. Behind the hernial sac is the fascia lata, and the sac rests in the hollow between that part of it which covers the crural vessels and that which passes over the pectineous and triceps muscles, so that the fascia lata is situated posteriorly to the hernia." P. 6.

From the description we gave of the crural ring in the sound state of the parts, we cannot agree with Mr. Cooper in saying, that a *thin fascia*, which naturally covers the crural ring is pushed forward before the peritoneum, and, by also pushing forward the sheath of the vessels, these two parts are united to form the *fascia propria* of the hernia. The *fascia propria* we have found in the cases which we have examined to be perfectly distinct, and the world is indebted to the persevering industry and the acuteness of Mr. Cooper for the knowledge of it. All we wish at present to remark is, that Mr. Cooper's explanation of the manner in which it is formed is not quite satisfactory; and we cannot help thinking, that instead of saying a fascia covering the crural ring assists in the formation of the *fascia propria*, we would say that it is composed of the cellular lamellæ and fat, which lie over the crural ring, and of the proper sheath of the femoral vessels. It is from a want of knowledge of this bag, (he very judiciously remarks,) in which the crural hernia is contained, that the great difficulty in the operation has arisen, and that surgeons have, in some instances, returned the hernial sac unopened, supposing it to be the intestine. We have seen repeatedly much confusion and embarrassment arise during the operation for femoral hernia, from the surgeon not being able to discriminate between the different coverings of the intestine and the intestine itself; and even in dissection after death, we have observed in old hernia the sac so much thickened, and so easily divisible into a number of

layers, and so vascular, that it was almost impossible to distinguish the peritoneal covering from a portion of intestine.

This part of the pathology of hernia appears to us of so much importance to the practical surgeon, that we have copied, in a simple outline, the sixth figure of Plate IV: It represents the dissection of a small crural hernia.

a Seat of the symphysis pubis.

b Seat of the spinous process of the ilium.

c Tendon of the external oblique muscle.

d Internal oblique and transversalis.

e Fascia of the transversalis. *f* Tendon of the transversalis.

g Inner portion of the fascia transversalis, passing to unite itself with the tendon.

h The crural arch. *i i* Round ligament.

k Round ligament passing into the abdomen.

l Crural artery. *m* Crural vein.

n Origin of the epigastric artery.

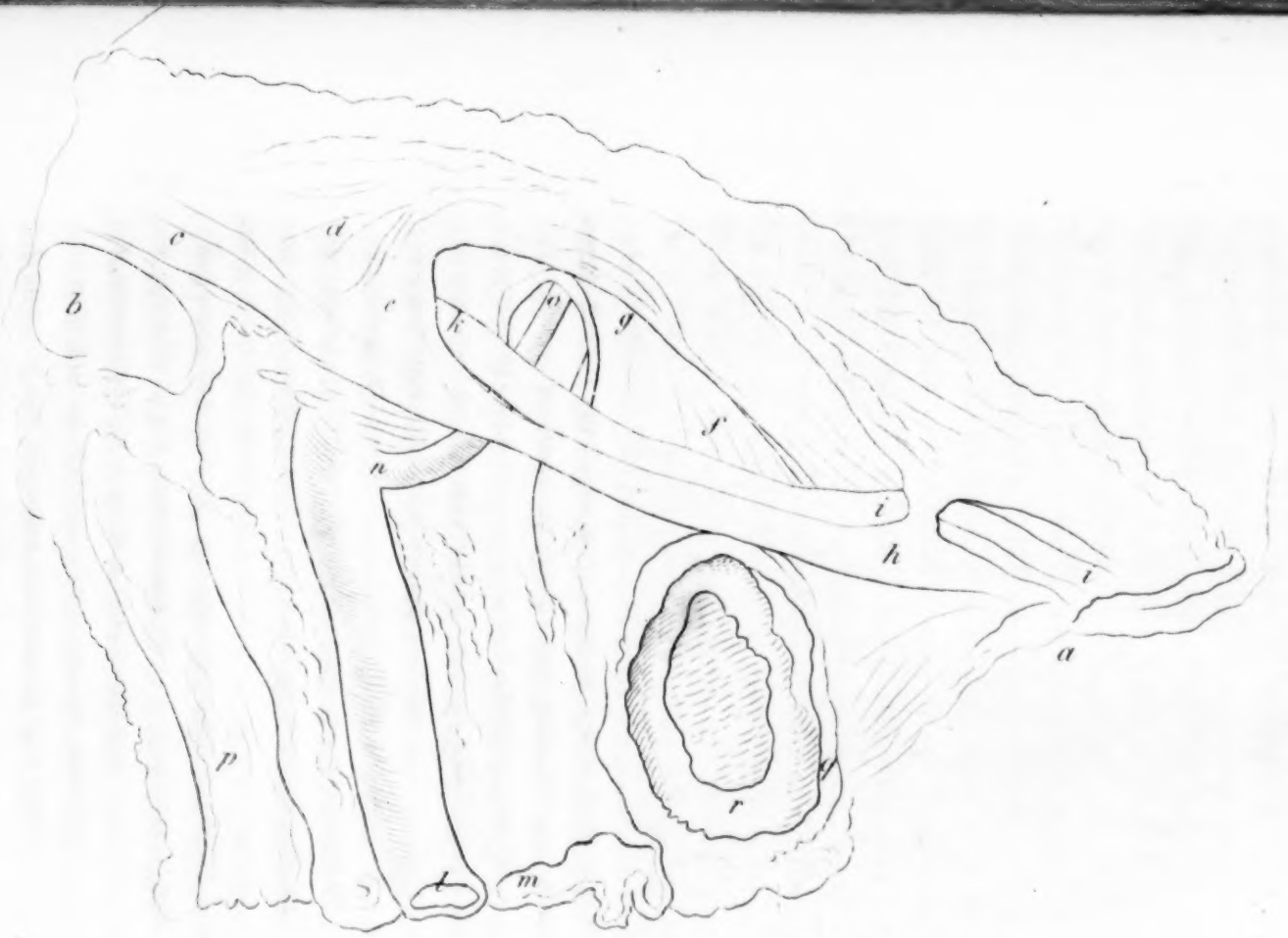
o Course of the epigastric artery behind the round ligament.

p Crural nerve. *q* Superficial fascia.

r Fascia propria of the crural hernia, the hernial sac having been drawn into the abdomen to show this fascia distinctly.

Our author next proceeds to give an account of the *diagnosis* of crural hernia from inguinal hernia, from psoas abscess, inflamed glands, varicose crural veins, steatomatous tumor, hydatid tumor; the cause of its greater frequency in the female; the contents of the sac, and the causes which most frequently bring on the disease. It is of much consequence not to confound the inguinal with the crural hernia, as both the taxis and operation are very different in each, and Mr. Cooper mentions several instances where this mistake occurred. It is by no means unusual to hear of a crural hernia having been mistaken for an inflamed gland, or an inflamed gland for a hernia; and this error is the more likely to happen, as in almost all cases of crural hernia, one or more glands are inflamed and swelled, and, in many instances, they so surround the herniary tumor as to prevent it from being felt.

Taxis.—It is of equal, if not more importance to attend to the position of the patient in making the attempt to reduce the fe-



Outline of a Hernial Sac shewing the Facia Propria described by Mr Cooper.

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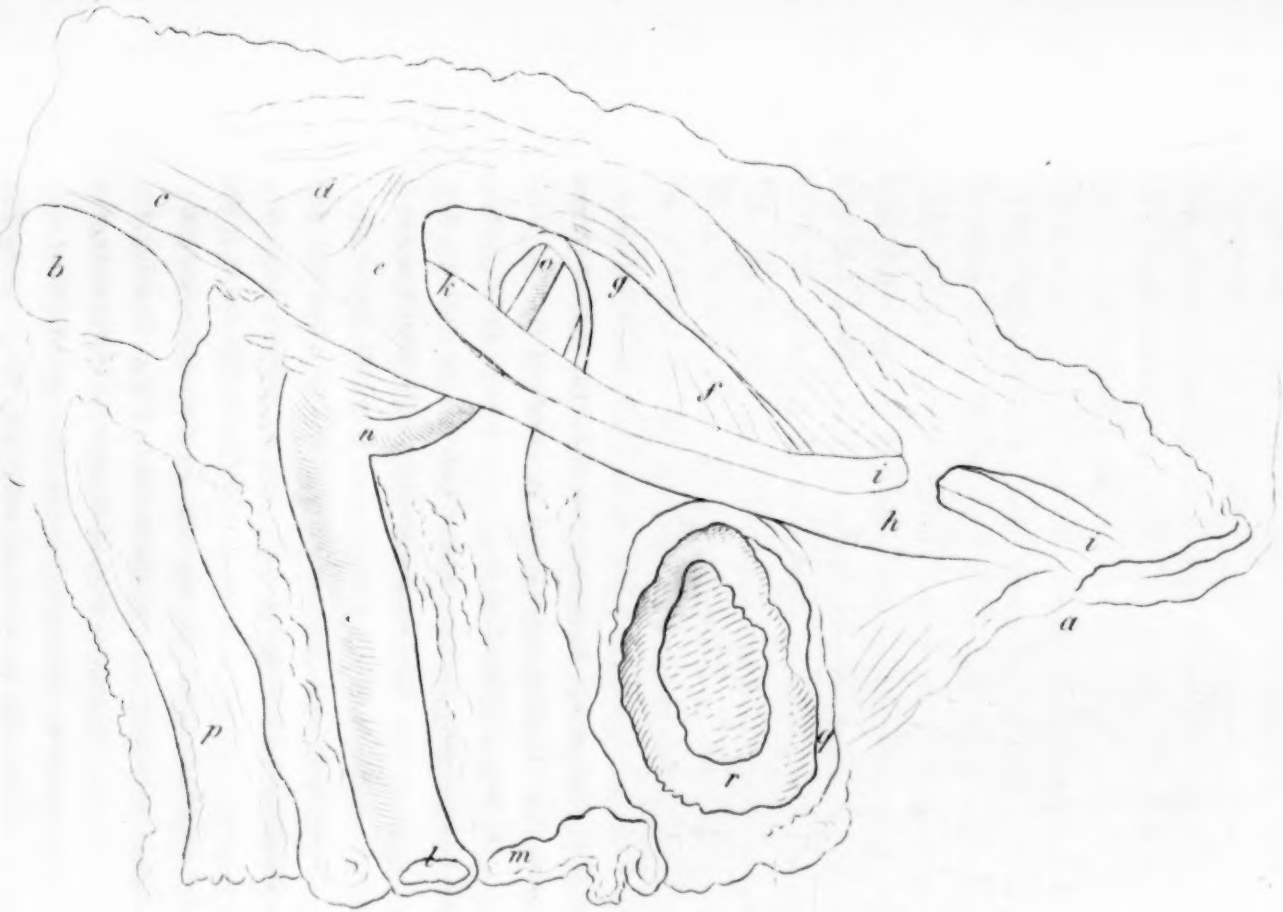

Outline of a Hernial Sac shewing the Facia Propria described by Mr Cooper.

moral than the inguinal rupture. "The shoulders should be elevated, and the thighs bent at right angles with the body; but even this posture produces little effect, unless, at the same time, the knees are brought together." In this position "the surgeon is to place himself over the body of the patient, and putting both his thumbs on the surface of the tumor, he is to press gently directly downwards, as if he was endeavouring to press the tumor into the thigh, rather than towards the abdomen. If this pressure is steadily kept up for some minutes, till the surface of the tumor is brought even with the line of the crural arch, the hernia may then be pressed towards the abdomen, and will return into that cavity." The truss for the crural hernia is much the same as that recommended for the inguinal; and the same general rules should be carefully attended to in its use and mode of application. It is only necessary that the pad be inclined more downwards, as, from the distance between the abdominal and crural rings, it is necessary that the bearing of the pad of the truss should be from one half to three fourths of an inch lower down than in inguinal hernia, and should also press on the upper part of the thigh. "For this purpose, it is best that the pad of the truss be bent somewhat downwards; and not as in the inguinal truss, contained nearly in a line with the spring."

Strangulated Crural Hernia.—In our review of the first part of Mr. Cooper's work, we took notice of the difference of opinion among surgeons, about the time the operation should be had recourse to. In general, the progress of the inflammatory symptoms is much more rapid in the crural than in the inguinal hernia, and it is therefore more necessary to guard against delay. Whenever a fair trial has been given to the usual means recommended for reduction, if the inflammatory symptoms are violent, the operation should be performed, the surgeon always keeping in mind, that the danger of the operation is in every case lessened in proportion as it is performed early; whilst, on the other hand, it is almost universally observed, that it is unsuccessful only when it is performed too late, or when insufficient means have been employed to remove the inflammatory symptoms, which had previously taken place.

"The incision of the integuments is to be begun an inch and

a half above the crural arch, in a line with the middle of the tumor, and extended downwards to the centre of the tumor below the arch. A second incision, nearly at right angles with the other, is next made, beginning from the middle of the inner side of the tumor, and extending it across to the outer side, so that the form of this double incision will be that of the letter T reversed.

"The first incision exposes the superficial fascia, which is given off by the external oblique muscle, and which covers the anterior part of the hernial sac; but if the patient is thin, and the hernia has not been long formed, this fascia escapes observation, as it is then slight and delicate, and adheres closely to the inner side of the skin. When this fascia is divided, the tumor is so far exposed, that the circumscribed form of the hernia may be distinctly seen, and a person not well acquainted with the anatomy of the parts, would readily suppose that the sac itself was now laid bare. This however, is not the case, for it is still enveloped by a membrane, which is the fascia that the hernial sac pushes before it, as it passes through the inner side of the crural sheath. This membrane, the fascia propria, is to be next divided longitudinally from the neck to the fundus of the sac; and if the subject is fat, an adipose membrane lies between it and the sac, from which it may be distinguished, by seeing the cellular membrane passing from its inner side to the surface of the sac.

"This is, in my opinion, the most difficult part of the operation, for the fascia propria is very liable to be mistaken for the sac itself; so that, when it is divided, it is supposed that the sac is exposed, and the intestine is laid bare; following upon this idea, the stricture is divided in the outer part of the sac, and the intestine, still strangulated, is pushed with the unopened sac into the cavity of the abdomen.

"The hernial sac being exposed, is to be next opened; and to divide it with safety, it is best to pinch up a small part of it between the finger and the thumb; to move the thumb upon the finger, by which the intestine is distinctly felt, and may be separated from the inner side of the sac; and then to cut into the sac, by placing the blade of the knife horizontally. Into this opening a director should be passed, and the sac opened from its fundus to the crural sheath."

It often happens, that this operation is sufficient to allow the strangulated intestine to be reduced, for it is now completely exposed; and it is much more under the command of the surgeon's fingers, than when enveloped in its different coverings. If, however, the contents of the sac are so much swelled, that they cannot be made to pass through the neck of the sac into the cavity of the abdomen, it becomes necessary to divide the stricture, and this, Mr. Cooper observes, may occur in three different situations:

1st, In the crural sheath, and semilunar or lunated edge of the fascia lata.

2d, At the posterior edge of the crural sheath.

3d, In the mouth of the hernial sac and fascia which covers it.

The seat of the stricture being ascertained by the introduction of the point of the finger, it is to be divided in a direction "obliquely inwards and upwards, at right angles to the crural arch," of a sufficient extent to liberate the intestine. But we may remark, that in every operation for hernia, of whatever kind, the business of the surgeon is, to divide the part forming the stricture and impeding the return of the strangulated intestine,—this must be divided; and if he divide no more, it is of little importance in what direction the incision be made. Indeed, we have seen cases where the stricture was so great, that it was with much difficulty that the nail of the surgeon's finger could reach underneath it, and he was obliged to divide it in any part where he could get the knife introduced and in any direction.

The varieties of the crural hernia, which Mr. Cooper mentions, arise either from the superficial fascia giving way, so as to allow a portion of the tumor to pass before it, thus dividing the tumor into two parts, or from the tumor not quitting the sheath for the crural vessels. Another variety occurs, where the tumor is formed "in part within the sheath, and also in the common way;" in one case, the author found the umbilical artery on the outer side of the sac. Another, and perhaps the most important variety, is, where the obturator artery comes off in a common trunk with the epigastric, and passes round the neck of the hernial sac.

This distribution of the arteries is by no means unusual; but,

in general, the common trunk of the two arteries is so short, that the obturator passes behind the sac. Mr. C. mentions only three cases, one which occurred to Dr. Barclay, and two which were communicated to him by Mr. Wardrop, where the common trunk was so long as to surround and pass anterior to the mouth of the sac.

Mr. Cooper, after making some very pertinent remarks on the operation inwards, or that proposed by Gimbernat, and his reasons for not adopting it, concludes the subject of crural hernia by illustrating the whole with very instructive cases.

Mr. C. has added a communication from Mr. Burns, demonstrator in anatomy at Glasgow, in which, besides remarks on some peculiarities in the structure of the parts concerned in hernia, he gives an account of a case of abdominal inguinal hernia, similar to those mentioned by Petit and the late Dr. Hamilton of Glasgow. In all these cases, the intestine entered the *internal* inguinal ring, and, instead of passing along the inguinal canal, and coming out at the *external* ring, passed directly through the abdominal parietes, opposite to the internal opening. Such cases occur either in consequence of an original deficiency in the number of transverse tendinous bands, which unite the columns of the tendon of the external oblique, or from their being lacerated.

Umbilical Hernia.—After having taken notice, that the peritoneum is as complete behind the umbilicus as at any other part of the abdomen, our author concludes, that the sac, in umbilical hernia, is formed of peritoneum, except in cases where that membrane is lacerated. He then describes the appearance of the disease, and the varieties to which it is subject; its frequency in children, its diagnosis, and the various causes from which it most usually proceeds. These are pregnancy, obesity, ascites, large funis, or a deficiency in the abdominal muscles.

In attempting to reduce this species of hernia, the same attention is necessary in relaxing the abdominal muscles, by elevating the shoulders, raising the pelvis, and bringing the thighs at right angles with the trunk.

"In making the reduction, the surgeon grasps the tumor in his hand, and directs it upwards, as the opening which leads from the swelling into the abdomen is not directly in the

centre of the tumor, but a little above it, and he then kneads the neck of the tumor with the finger and thumb of the other hand; but if the swelling is small, and projects only from the abdominal muscles to the navel, without spreading on each side, a direct pressure may be made upon the surface of the tumor with the thumbs, so as to force it into the cavity of the abdomen."

After the hernia is reduced, a proper bandage should be applied; and Mr. C. recommends one exactly similar to that used for the inguinal rupture. This, we know from experience, answers most completely, and it is by far the most simple contrivance. The spring should reach from the pad round to the back, a little further than the spine.

When the hernia is very large, especially when the belly is also pendulous, broad thin pads are recommended, which both keep back the hernia, and give a support to the abdomen. In infants, Mr. C. has found the following plan answer:—

"A small section of an ivory ball is laid on the navel; over this is to be applied a piece of adhesive plaster, of the size of the palm of the hand, and a linen belt over the whole round of the abdomen. As soon as the child begins to walk, two thigh straps are to be added to the bottom of the belt, to prevent it from slipping. If this application be carefully persisted in for a length of time, the cure of this disease is very sure to be effected."

Indiscretion, both in the quantity and quality of food, is the most frequent cause of strangulation in this species of hernia; and Mr. C. has often observed quantities of ham, and lumps of fat, pass through indigested in those cases where an artificial anus was formed.

Our author has found the tobacco glyster, as directed in Part I. of his work, to be a most useful remedy in the strangulated umbilical hernia. And when that fails, he advises the use of cold applications. The symptoms which render an operation necessary in this species of hernia are the same as those which occur in the other species. Mr. C. does not employ the common operation, by a longitudinal incision of the tumor, but has adopted the following:

"As the opening into the abdomen is placed towards the

upper part of the tumor, I began the incision a little below it, that is, at the middle of the swelling, and extended it to its lowest part; I then made a second incision at the upper part of the first, and at right angles with it, so that the double incision was in the form of the letter T, the top of which crossed the middle of the tumor. The integuments being thus divided, the angles of the incision were turned down which exposed a considerable portion of the hernial sac. This being then carefully opened, the finger was passed below the intestines, to the orifice of the sac at the umbilicus, and the probe-pointed bistoury being introduced upon it, I directed it into the opening at the navel, and divided the *linea alba* downwards, to the requisite degree, instead of upwards, as in the former operation.

"When the omentum and intestine are returned, the portion of integument and sac which is left at the upper part undivided, falls over the opening at the umbilicus, covers it, and unites to its edge, and thus lessens the risk of peritoneal inflammation, by more readily closing the wound."

In cases where the hernia is very large, and consequently where the risk of peritoneal inflammation is considerable, Mr. C. has, in several cases, instead of laying open the sac and the abdominal cavity, and exposing a large quantity of intestines, made a small incision of the integuments, at the neck of the sac, and divided the stricture.

In *Ventral Hernia*, whether the disease has been occasioned by an enlargement of any of the apertures of the bloodvessels, from original malconformation, from the ulceration of muscles, or from wounds, the same general rules are laid down as those in the other species.

The subject of *Vaginal*, *Pudendal*, *Perineal*, *Thyroideal*, *Cystic*, *Ischiatic*, and *Phrenic Hernia* our author discusses as minutely as the subject will allow; but these species occur so seldom, and as Mr. C.'s information would not admit of a short analysis, we must refer those to the work itself who wish for full information on these subjects.

Our author mentions one remarkable case of *Mesocolic Hernia*, in which the whole of the small intestines were contained within a sac formed between the layers of a dilated portion of the mesocolon.

He has also found the intestines strangulated, from apertures in the omentum, mesentery, or mesocolon,—from adhesions having formed in consequence of inflammation, leaving an aperture in which a portion of intestine becomes confined, by membranous bands, formed in the mouths of hernial sacs, which becoming elongated by the frequent protrusion and return of the viscera, surround the intestine, so as to strangulate it within the abdomen when it was returned from the hernial sac.

Mr. C., in completing this work, has added many useful and ingenious observations to the anatomist, and he has contributed so much to a very important part of surgery, that he has left little more to be done in it. A most commendable zeal in a profession which interests the happiness of so many individuals, seems to have actuated this gentlemen in pursuits where much difficulty and many obstacles opposed his progress, and which nothing but indefatigable industry and perseverance could overcome.

The anatomical description, and the most remarkable morbid appearances are very fully illustrated in seventeen engravings. The opinion we formerly gave of these as works of art, we might again repeat, as it may apply to the greater number of those contained in the volume before us. Although, however, the drawings do not breathe the spirit of a great master, yet they are executed with great neatness and truth, and much care seems to have been taken in the choice of proper subjects. We ought, however, to caution our readers against one error into which Mr. C. has inadvertently fallen. He wishes the plates to be referred to for taking *measurements* of the relative situation and distances of parts. Had they been accurate *maps* instead of *views*, this might have been done, but in the art of drawing it is impossible; for as the different parts are seen in perspective, their apparent distance and situation must vary according to the situation of the eye of the observer.

In the notice we have taken of this valuable work, our chief object has been to extract some of the important information and the judicious practical lessons it contains, not to offer many critical remarks; and the analysis we have made, will, we hope, rather than satisfy the arduous in their professional inquiries, induce them to consult the work itself.

Anatomie et Physiologie du Système Nerveux en général, et du Cerveau en particulier, avec des Observations sur la possibilité de reconnoitre plusieurs dispositions intellectuelles et morales de l'homme, et des animaux, par la configuration de leurs têtes. Par F. J. Gall *et* G. Spurzheim. *Premier Volume. Anatomie et Physiologie du Système Nerveux en général, et Anatomie du Cerveau en particulier.* 4to. pp. 351. Paris, 1810. Atlas en folio, contenant dix sept planches.

[From the London Medical Review for July, 1811.]

The anatomy and physiology of the brain have of late engaged much of the attention of the anatomists and men of science on the continent. Alleged discoveries of facts not before noticed in the structure of this organ, together with a new mode of explaining its functions, have already formed the basis of a complete system of physiology of the brain, in which the special functions of the several parts of which it is composed are regularly determined and explained; and which includes, as essentially connected with these functions, the natural history of our faculties, our dispositions, in short of all our intellectual and moral qualities.

Of the philosophers who have hitherto reasoned on the powers of the brain, the greater part have contented themselves with considering it as the common centre of sensation, and the general organ of the will: whilst others, without possessing any accurate knowledge of its structure, have ventured to assign particular functions to particular parts, and have fallen into the greatest absurdities. Of the numerous anatomists, and physiologists who have diligently examined this organ in various states both of health and disease, the majority, avoiding metaphysical discussions, have confined themselves to attempts at illustrating its importance in the animal system: some, and those of the greatest eminence, struck by the observance of various phenomena in the course of their researches, were convinced of the existence of different qualities both physical and intellectual as connected with special parts of the brain, but their notions on the subject were so confused and their judgments so much warped by the metaphysical opinions of the day,

that they had the effect rather of throwing the subject into shade, than of diffusing upon it any light which might direct the attention of future inquirers. There are yet a few who think that every thing has already been done; that after the numerous dissections which have been made, and descriptions which have been given of the structure and appearances of the brain, nothing remains to be discovered, and that all further attempts at investigating its functions must be idle and full of metaphysical uncertainty.

It is natural to suppose that a doctrine which struck at the root of the commonly received opinions, and which professed and promised so much, would be received with jealousy, and freely canvassed by those, whose self love it so sensibly touched, and against whose theories it so strongly militated. We find accordingly that the opinions of Gall, on their first promulgation in Germany, whilst they had various supporters, who adopted them without examination and bestowed on them unbounded praise, met also with strenuous opponents, who supporting themselves by some reasonable objections to subordinate parts of the system, scrupled not to reject the whole as false and absurd. Others unable to oppose facts by argument, broke out into lamentations on the horrible moral consequences which must follow their adoption. The greater number of the adversaries of this physiology, without at all troubling themselves to examine the subject, attacked it by ridicule, in matters of science the weakest of all the weapons of criticism.

The doctrine of Gall which excited so lively an interest on the continent, has been received so coldly in this country, that but few we believe have any accurate knowledge of the principles on which it is founded: sure we are that no one has made any attempt to investigate it. And whilst his peculiar physiological opinions have been but very imperfectly understood, and mentioned only to be laughed at, his successful labours as an anatomist, the basis of his physiological opinions, have been wholly overlooked. In France his pretensions have met with a more favourable reception; and his anatomical observations, more particularly, have been patiently and deliberately examined by men who hold the first rank amongst the anatomists and physiologists of the present day.

The doctrines of Gall with regard to the structure of the nervous system, and more particularly of the brain, totally change the common ideas and methods of investigating the subject. We should observe here however, that he does not pretend to have discovered many new facts; the chief merit that he claims consists in the connexion which he has been the first to establish between known facts, and in the general conclusions which he has deduced from them. His doctrine concerning the special functions of different parts of the brain is in many respects new, ingenious, and important. Its intent is to prove that there are in man and animals, innate qualities and dispositions, connected with certain organs which are situated in the brain: that the brain is not so much the general organ of animal life, as the place in which the individual organs are collected, each innate quality having its special organ: that these organs show themselves on the surface of the brain, and form eminences, marked by corresponding risings of the cranium; and that from an observance of these last, we may under certain limitations, determine the existence of such organs, in a greater or less degree of development, answering to the greater or less degree of energy in the intellectual or moral disposition with which they are connected. Its merits must rest on the number and importance of the facts which can be brought forward for its support. We shall not at present, however, enter on this part of the subject, as the volume which is to contain his own detailed account of the opinions is not yet before the public. The judgment already passed on them has been made up from accounts published by some of his pupils, none of them entirely acknowleged by himself.

It is now more than twenty years since Dr. Gall first made known his peculiar opinions, which by his own account, he began to form very early in life, from observations made on the dispositions and talents of his school-fellows. As he advanced he lost no opportunity of repeating, and of trying the truth of his remarks, and became more and more convinced of their correctness. Aware that any doctrine explanatory of the functions of the brain must necessarily be very imperfect, if not strictly and intimately connected with the structure of the organ, he entered on minute anatomical ex-

aminations of its substance. His opinions, as he gradually made them known, became the subject of much discussion in Germany, where they have been the cause of a warm literary warfare.

It was not long before Dr. Gall, perceiving the interest his doctrine had excited, entered on a regular course of lectures on the physiology of the brain, in which he enlarged more particularly on the existence of special organs, for different intellectual and moral qualities, and on the possibility of ascertaining their existence, by the form of the head, in the living individual. His lectures given at Vienna, were exceedingly popular, and very numerously attended; when he was interrupted by an order from the government, and prohibited from lecturing before any but foreigners. The reason assigned was, that his doctrine was dangerous in its tendency, and led to materialism and atheism. Weak indeed must that establishment have been in church and state, which could be seriously alarmed by the harmless doctrines of Dr. Gall. They savoured no more of materialism than any other of the numerous physiological systems of the functions of the brain, which had never probably been discussed out of the precincts of the schools, certainly had never been thought dangerous enough to require the interference of the rulers of an empire; and this too, in the nineteenth century.

Gall soon after quitted Vienna, in company with Dr. Spurzheim. They visited the principal cities in the North of Germany, giving public lectures, conversing with men of science, inviting criticism, and always diligently labouring to add to their store of facts. They every where met with a most flattering welcome; and their designs were forwarded in every way by sovereigns and men in power, as well as by the learned, and artists of every description. Among many distinguished physiologists who had now taken up the subject as one of considerable interest, we find Hufeland, Walther, Loder, Reil, and Sœmerring; the two former as opponents, the latter as supporters of most of his opinions. All of them men of extensive knowledge, and high authority in anatomical and physical science. It is by such men that the question must be finally determined.

We next find Gall and his zealous assistant Spurzheim at Paris, in which place also they have continued their labour of collecting and arranging materials for the present work. They have given repeated demonstrations of their anatomical discoveries to all who expressed any desire, or showed any intention of investigating the subject, and have left no one to complain of a want on their part either of liberality, industry, or talent. In the beginning of 1808, they presented to the Institute, in matters of science one of the first tribunals in Europe, a memoir intitled "Recherches sur le Systeme Nerveux en general, et sur celui du Cerveau en particulier." It was referred for examination to MM. Tenon, Portal, Sabatier, Pinel, and Cuvier, who drew up in consequence a most elaborate and excellent report, which contains within a short compass the essence of Gall's opinions relative to the anatomy of the brain, together with many valuable remarks on them by the reporters. This report has been translated, and published in the fifth volume of the Edinburgh Medical Journal, and we cannot too strongly recommend it to the notice of all such as are desirous of inquiring into the structure and probable functions of so important an organ.

The reporters in speaking of some of the motives which induced them to undertake an examination of the memoir, say, "the consideration of the importance of the functions of the nervous system, and of the ignorance which still exists, concerning several points of its structure, notwithstanding the numerous examinations to which it has been subjected, fixed our determination. Whoever flatters himself that he is able to throw any light on a subject so interesting, and so obscure, is, in fact, entitled to be heard with attention, by a body such as ours; and we should be wanting in our first duty, if our examinations were not conducted with the greatest attention, and strictest impartiality." "We invited therefore Dr. Gall and Dr. Spurzheim to our conferences. They readily dissected the brain in our presence, and we dissected it before them; we afterwards reexamined by ourselves the observations they communicated to us; lastly, we endeavoured for a time to adopt their manner of veiwing the subject, and to give a clear and precise abstract of it, which we submitted to them, that

we might be satisfied whether we had rightly comprehended their ideas."

Indeed, they appear to have taken every means of ascertaining what was new, or just, in the ideas of the authors of the memoir, and to have used every precaution before they formed their opinion. This opinion is, with but few exceptions, much in favour of our authors, allowing them the merit of having recalled to our attention, and again demonstrated neglected facts, of having discovered new ones, and of having drawn some just and important conclusions from both.

The memoir above alluded to has since been published by its authors with some remarks on this report. It is confined wholly to anatomical investigations.

We confess that it has excited our surprise, to find so little notice taken of the memoir, or of the report, by the men of science, or by the anatomical teachers of this country, the latter of whom, still go on slicing the brain as they were wont to do. It has always appeared to us, that the method of demonstrating the brain at present commonly employed in our anatomical schools, is defective in every sense. What idea could possibly be formed of the appearance or structure of the thoracic or abdominal viscera if the dissection were performed in a similar manner? Who could divine the functions of the heart or stomach from a demonstration of thin horizontal slices? The principal effect of this practice is, to puzzle the student with hard names of parts imperfectly shown, the structure and relations of which are neither pointed out nor sufficiently explained. Add to this, from the manner in which the subject is generally treated, the student is soon impressed with the idea that a minute knowledge of the anatomy of the brain can never be of much service to him in the practice of his profession, and that any time spent in acquiring it is but so much thrown away.

Of the work now before us the first volume only has been received in this country: we believe it is the only one yet published. It contains a very detailed description of the nervous system, and of the brain; and is accompanied by a folio volume of plates, executed in a most masterly and finished manner. The second volume will contain the general principles of the

physiology of the brain; and the third the doctrine of the different organs.

It has been objected to Gall that he cautiously abstained from printing any of his opinions, and that he depended for popularity on the flattering accounts of prejudiced, and enthusiastic pupils. To this he answers in the preface to the present work: "Every one expressed a desire of seeing me put to the press my physiology of the brain; I was much inclined to do it, but from day to day a wider field opened to my view, and questions and problems continually multiplied. It is for these reasons, that, notwithstanding repeated solicitations, and many keen provocations, I always endeavoured to put off the time of publication. If I am enabled to recompense the reader by a more abundant harvest of observations, and by more positive conclusions, it is owing to this intentional delay, and to my regard for science."

A considerable part of the present volume is occupied by a long desultory preface, and an introduction. The former notices the various labours of the authors, and the obstacles which they had to overcome; and touches slightly on some of their physiological opinions. It betrays here and there an inclination in the composers to give us some fine writing, but at the same time contains many philosophical views of the functions of the nervous system.

"In this volume our principal object is the anatomy of the brain. But in order to give a connected view of the whole, and to enable the reader to understand more readily the laws of this the most noble part of the nervous system, we shall begin by treating of the more simple and lower order of systems, the ganglions and plexuses of the abdomen and thorax, and the nervous systems of the vertebral canal and of the senses. As we do not confine ourselves to give merely a detail of the mechanical structure of the brain, but connect with it our views of its physiology, we shall also join to our anatomical description of the inferior nervous systems some account of their functions."

"It has been said indeed that our doctrine on the structure of the brain has no necessary and immediate connexion with our doctrines of its functions, and the influence of the pro-

portionate size of its different parts; that all we have advanced on the structure of this organ might be equally either true or false, without thereby affecting the conclusions that may be drawn as to our physiological opinions. But this is separating anatomy from physiology; it is destroying all the relations between organs and their functions. It is certain that any doctrine of the functions of the brain would be false, if in contradiction to its structure: at the same time we do not pretend to explain any faculty by anatomical structure. Anatomy may serve to confirm, and to explain physiological truths; but it is seldom that it leads to a knowledge of functions, especially where the brain is the subject, the parts which have so much apparent resemblance to each other. Whoever is convinced that the structure of the parts of the brain has a necessary and immediate relation to their functions, will perceive that it is natural to unite these two objects, and to consider them as forming parts of the same body of doctrine. Since we attach so much importance to an exact knowledge of other organs, why not still more to that of the nervous system? Can there be in all the domains of natural history a study which concerns us more nearly than that of the organization of the brain?

"In anatomy our principal aim is to make known the organization of the nervous system, and particularly that of the brain; directing our attention in the first place towards the discovery of the organic laws of the nervous system; and in demonstrating them to set off from the more simple systems, and to ascend by degrees to the more complicated. In physiology, we endeavour, indeed, to determine the primitive faculties of the mind, the situation, and the external marks of their special organs; but however important a knowledge of these may be to the philosopher, or the physician, we are far from confining ourselves to this single object, only regarding it on the contrary as one of the best means of ascertaining with exactness the functions of the brain and of its several parts. Consequently we direct our attention in an especial manner to researches on the faculties of irritability, of sensibility, of voluntary motion, as well as on the functions of the senses, the will, or thought, as far as any of these qualities is dependent on conditions of matter. On every occasion we take

into our account, education, morals, legislation, the art of healing in general, as well as the knowledge and treatment of mental and moral affections."

We have allowed the authors to speak for themselves, that our readers might see the extensive route they have marked out, and the importance of the subjects which they intend to examine. We must excuse a little vanity in the following account of the opposition made to their doctrines, and of their final triumph. It is but a small part of what they say on this subject; we have many bitter complaints of the prejudices and jealousy of rival ānatomists, as well as self-congratulations on the impregnability of the principal parts of their doctrines.

"We have hitherto taken notice of all the objections made to our assertions, which have come to our knowledge, and we shall always do the same. The only thing we regret is that our antagonists do not take time to understand our doctrine before undertaking to refute it. The chief part of them attack rather some absurdities which have been attributed to us than our real opinions; a manner of proceeding which betrays motives of no honourable source rather than a zeal for truth. Any discovery however, would remain very incomplete, if it obtained universal assent too readily. For the success of its author, nothing is required but that he live under a government which does not believe that the safety of the universe may be endangered by publishing natural truths. It is the coward only who is intimidated by difficulties and trifling persecutions. The man who is conscious of being the supporter of a new truth, feels a new spur in every difficulty, in every obstacle a new subject for triumph. It is only by such opposition that he is enabled to view the subject in all its bearings; it is only by resolving doubts, and answering objections, that he arrives at the point of placing the principles of his doctrines in security against the reach of any attack. It is thus that we have succeeded in placing in the rank of confirmed and demonstrable knowledge, the discoveries which we have made on the structure, the arrangement, the connexions, and the relations of different parts of the brain; on the use of its two substances, as well as on the origin and direction of the nervous fibres. We believe that we have introduced order,

unity, and life into a study in which hitherto there has been so much disorder. Where nothing was seen but mechanical forms, we shall show the material apparatus of the functions of the mind. Without presuming to understand the essential principle of our faculties, we shall demonstrate the corporeal conditions on which these faculties depend."

We were struck by some true, and eloquent observations on the peculiar advantages and opportunities, enjoyed exclusively by medical men, of studying human nature; and on the probable good which might be hoped for from their experience, in regulating civil institutions. It is by such views that we are led to value more highly, and to allow more readily the importance and utility of an honorable profession.

In the introduction we have an historical account of all the various opinions which have been held on the physiology and anatomy of the nervous system; some observations on the importance of studying its functions, and on the obstacles which retarded this branch of knowledge. In the concluding paragraph we find a review of their proceedings, which we insert, in order to give our readers a favourable impression of the merits of men who have laboured so indefatigably. Without particularizing their anatomical investigations, they say—

"In order to remove every obstacle, and employ every means within our reach, we have combined with our previous researches, and with the fruits of many years' labour, all the information we have been able to collect in our travels. Our intercourse with a great number of distinguished men, and an observance of their predominant qualities, have furnished us with many luminous ideas. We have profited by the observations made by our auditors, and by our friends, and by the objections made by sceptics, and by our adversaries. It is thus that we are arrived at the knowledge of many difficulties and many defects in our doctrine. We have at the same time studied without ceasing the organization of men of great talents, and that of men of low talents, in order, by the comparison, the better to hit on the difference between them. We have collected innumerable facts in our visits to schools and the great establishments for education, in the hospitals for foundlings and for idiots, in the houses of correction and in prisons, in courts

of justice and at the scaffold. We have added very largely to our collection of busts, and of the skulls of men and animals. We have examined a great many anatomical cabinets, &c. And lastly we have compared our opinions and our maxims with those of ancient and modern authors."

"After the employment of all these precautions we hope to have the credit of being thought sufficiently prepared to treat of the anatomy and physiology of the nervous system, as well as the history of these sciences. We shall attach ourselves more particularly to the brain, as being of the most delicate and most perfect organization, and as the immediate instrument of the soul. These labours appear to us of the greater importance, as it is in this part that the knowledge of anatomists, physiologists, and philosophers, is the least advanced. For the same reason we think we have some claim to the indulgence of our cotemporaries, and of posterity, for the errors and imperfections that may be discovered in our works. They will not be attributed to a want of application, to the omission of any necessary means, but solely to the incompetency of our talents, and to the shortness of life. We shall ourselves point out some deficiencies in our doctrine; without doubt our successors will discover others, which at present we can see but in the distance."

Hitherto we have done nothing but introduce our authors, and we have been thus copious in our observations, to enable our readers to form a just estimate of their pretensions and labours. We have been anxious to vindicate the name of Dr. Gall from that ridicule with which, in this country, it is generally associated; to represent him not as the idle dreamer of craniology, but the author of deep and erudite researches into the anatomy and physiology of the brain. In our next, we shall give some account of the store of anatomical facts contained in the present volume.

[*Same subject continued.*]

[From the London Medical Review for October, 1811.]

It is so rarely our lot to meet with a book containing accounts of discoveries in the structure and functions of any part of the

human body, that we believe many of our readers would agree with us in entering at length into an examination of the important contents of the present volume. But it would be fruitless to endeavour to discuss the many subjects of which it treats within the necessarily short limits of a Review. It contains such detailed descriptions of different anatomical appearances; so many physiological disquisitions; so copious a history of the opinions of every eminent anatomist, as well as those more peculiarly their own; and such elaborate answers to the objections made to many of their conclusions, that it will be impossible even to give a view which shall include every point. We must confine ourselves to noticing the most prominent, and most interesting features of the discoveries and opinions contained in the present work.

Our authors begin with the examination of the intercostal or great sympathetic nerve, because in the more perfect classes of animals the ganglions and plexuses of the abdomen and thorax may be considered as forming the system of an inferior order of functions. Their opinions with regard to the origins of this nerve will wear no semblance of novelty to such as are acquainted with the writings of later physiologists. The opinion which regarded the brain as the origin and point of departure of all the nerves of the body, either immediately or by means of the spinal marrow, has been gradually departed from, in proportion as a better acquaintance with comparative anatomy, and a more accurate mode of reasoning in physiology have shown its inconsistency. Many anatomists have remarked in particular the impropriety of deriving the great sympathetic nerve from the brain, and have considered the ganglions as so many separate origins of it. Bichat has entered more at large on the same subject, and in establishing the ganglions as so many nervous centres, possessing each an independent action, or influence, exerted on the nervous filaments passing from them to different parts, has given us occasion to admire the clearness of his conceptions, and the expressive language by which his theory is at once supported and adorned. To strengthen these opinions, our authors remark that the nervous systems of the viscera of the lower classes of animals, who possess neither brain or spinal marrow, may be looked on as analogous to simi-

lar systems in the higher classes, in whom similar functions are carried on. They correspond to the nervous plexuses of the abdomen and thorax, and to the series, more or less interrupted, of the ganglions of the sympathetic nerve; admitting this, it is demonstrated that this apparatus exists independently and of itself; and that the nervous filaments which connect it with the nerves of the head and spinal canal are only branches of communication. We are certainly fully disposed to consider the nervous systems of the abdomen and thorax as so many distinct systems, having a reciprocal action on each other, and on the brain, and exercising different functions according as they give origin to different nerves.

The authors throughout this work make the greatest use of comparative anatomy. It is from analogies drawn from that source that many of their opinions originated. The manner in which it is brought forward in the present instance is ingenious, and the argument is well followed up. With regard to the knowledge derived from comparative anatomy in elucidating the structure and functions of the nervous system in general, we shall here for once observe, that although we must be cautious in admitting as proofs of an uncertain physiological opinion, examples taken from animals differing so widely as an acephalous molluscus and man, yet where the series of facts with respect to the apparent organization of parts can be clearly traced from the simple to the more compound, it must be allowed to constitute no inconsiderable proof. Nor can we see by what reasoning some physiologists are guided, who in investigating the structure of other parts of the animal system, constantly refer to comparative anatomy as a fruitful field of convincing facts, and as the surest means of avoiding errors in physiology: yet who, when the brain is the subject, will not for a moment allow any conclusions to be valid, though drawn from analogies as plain as those on which they build so much. Scarpa is allowed to bring forward the organ of hearing from the cuttle-fish, through all the different classes of animals, to explain its functions in man. We can see no reason why the same excellent plan should not be followed in the present case. There is nothing so very distinct in the brain and nervous system of man as to exclude all analogies drawn from other

living creatures. Our advancement in anatomical knowledge is at once stopped if deprived of such a clue. The more simple the structure of an animal, the more easy is it to examine its nervous system; the greater the probability of making, or of confirming discoveries, than if we examined only animals of a more compound order, in which so many more parts are intermingled with one another. In man, the brain, considered exclusively, is certainly more developed, more perfect in structure, than in any other creature; but we are led to the discovery of this perfection in structure by comparative examinations, in the same manner as we are led to discover the perfection of its powers. In other animals various parts of the nervous system are traced in stronger lines than in man, the whole apparatus more plainly developed, the nerves proportionally larger throughout, and their origins and terminations more distinct.

The authors proceed to investigate the functions of the systems of the great sympathetic nerve, and in conclusion give the following statement of their opinions on the nature and use of the ganglions. This we have extracted as expressive of their ideas on the origin of the nervous or medullary fibres; a point of their doctrine which has been warmly assailed, and which applied to the higher parts of the nervous system is the very keystone on which the main part of their opinions rests.

"We have observed in the inferior classes of animals two substances forming the nervous system; the greyish gelatinous substance, and the nervous threads. The viscera are provided with collections of this gelatinous matter, and we may distinctly see nervous threads arising from them, and passing to different parts where they are lost. And as these collections of the gelatinous substance form knots or swellings, they are called ganglions. The more considerable the mass of gelatinous substance, the stronger and the more numerous are the nerves which proceed from it. We suppose that the nervous filaments are originally produced in this substance, and that they are nourished by it. The same conditions being found in the plexuses and ganglions of the higher classes of animals we should look on these also as the origins of nerves. They are penetrated by numerous vessels, and exhibit every where the gelatinous substance, pulpy, unctuous, and of a reddish grey

colour. They take different forms; and are of different consistence according to the distribution of the cellular tissue. The gelatinous substance is sometimes generally diffused, and the nerves which arise from it form a plexus; at others, it is collected in smaller masses in the plexuses. The difference in form, consistence, and colour, shows that these qualities are not essential conditions in the nature of a ganglion; they simply indicate modifications of structure."

The second section includes the history of the nervous system of the vertebral column.

The term spinal marrow has been used in a vague manner by different authors. The natural limit, in giving a description of it, appears to be the edge of the foramen magnum. Accordingly in the present inquiry it is supposed to end at the lower extremity of the pyramidal bodies, although it is by no means inferred that the structure of the spinal marrow is changed immediately as it becomes contained within the cranium. It is to be wished that the term marrow were discarded, as it is not applicable either to the grey gelatinous substance which occupies the central part, or to the nervous fibres which form the outer part of the cord.

In common language the spinal marrow is spoken of as a prolongation, or continuation of the medullary substance of the brain and cerebellum. There are however, numerous facts which prove that its existence is independent of these parts. In speaking of the systems analogous to the spinal marrow in the lower classes of animals, they observe that, in the worms and caterpillars there are as many origins of nerves, or ganglions, as there are rings or segments; that these knots of cineritious matter are joined to each other by nervous branches; and form a nervous cord, studed at intervals, with small tumors of various forms and size but all divided into two parts, so that all is double. The number and size of the nervous threads proceeding from these swellings is always in a direct ratio to the size of the latter. In fishes, amphibia, and birds, the principle is the same: there is no essential difference; only the ganglions are in general closer to each other, forming with their uniting bands, a cord nearly equal in size, but swelling out in distinct knots wherever large nerves pass off; and if

closely examined we may perceive throughout alternate swellings and contractions. This is very visible in the fowl, each pair arising from cineritious substances, and communicating with those on each side by connecting branches. The same law obtains in mammalia and in man, although the facts are not so immediately evident. The alternate swellings corresponding to the origin of the nerves may be seen by removing the spinal marrow, detaching the arachnoid coat, and observing the profile when held against the light. The largest swellings are where the nerves of the extremities go off, but they are sufficiently visible throughout, the line being more or less undulating.

The interior structure of the spinal marrow, though often examined, does not appear to be well understood. There is evidently an anterior and a posterior longitudinal fissure. Our authors deny the existence of any lateral ones, in which they are certainly correct. The anterior fissure is more visible and larger, the posterior penetrates more deeply. The anterior fissure has also the peculiarity of being interrupted at the beginning of the medulla oblongata by the decussation of the pyramidal eminences. The two halves of the spinal marrow are united at the bottom of each fissure by a nervous layer, or commissure; the fibres in the anterior being transverse, and in the posterior directed lengthwise. They consider these nervous layers as the apparatus by means of which the two halves of this part of the nervous system communicate with and act on each other, in the same manner as takes place in the brain and cerebellum by their several commissures. This we think will be readily admitted.

It has been the common opinion that the spinal nerves are derived only in a secondary way from the spinal marrow, their original production being referred to the brain. They are here considered as originating in the cineritious substance of the spinal marrow. This substance is found in the central parts of each side of the cord, joining "two arched streaks, which pass from the middle layer towards the anterior and posterior surface, so that the two lines of each half form two arcs, the convexities of which terminate at the median layer." All the

nervous fibres follow the course of these striæ from within to the surface, throughout the whole length of the spinal marrow, so that we have on each side two rows of nerves, an anterior, and a posterior.

The nervous systems of the vertebral column serve to the brain as the instrument for voluntary motion, and the conductors for sensation. The same nerve is employed for each purpose, although by disease sometimes one power is lost and the other retained. The manner in which this is produced is not explained. The diseases of these systems are not well understood. The injury of one side of the spinal marrow is felt on the same side and does not necessarily extend its effects to the other; there is therefore no crossing of nerves. But there certainly is a communication between the origin of each pair; which fact may throw some light on the sympathies of corresponding parts on the two sides of the body.

The existence of a canal in the middle of the spinal marrow has been casually mentioned by several writers. Our authors give the following account of the appearance. They first observed it in examining a case of spina bifida attended with hydrocephalus:—

"In order to know positively if there existed any communication between the sac and the internal part of the spinal marrow, we divided the latter transversely in the neck: it exhibited its natural appearance. Blowing gently through a pipe on the cut surface, we perceived an opening as large as that of a common quill in each half of the spinal marrow, separated by the commissure. They were the openings of two canals, which, by dividing lower and lower down, were found to terminate about three inches above the end of the spinal marrow, having no communication whatever with the sac. Doubtful if this appearance might be the effect of disease, we examined this part in children and in adults. We found the same structure with this difference only, that in the adult and older children these canals did not open so freely as in the younger subjects. The structure is most easily shown in children just born. In other respects they are alike. If we continue to blow gently into these canals from below upwards; and if, after having opened by this operation the length of six or eight lines in the

spinal marrow, we only cut off four or five in order to maintain the opening, we may follow these canals through the medulla oblongata, the tuberculum annulare, the crura of the brain, and as far as what are called the optic thalami, in which they form a cavity as large as an almond, when blown up: they are closed at the anterior margin of the thalami, at the commencement of the corpora striata. They have no communication with the cavities of the brain."

Our authors give many sound reasons for considering these as natural canals, not formed by artificial expedients. Indeed there can be no reason to doubt a fact, which is made very apparent by gentle management. They are easily shown to within a short distance from the lower end of the spinal marrow.

At the end of this section several anatomical and physiological corollaries are introduced, containing the results of the inquiries into these parts of the nervous system. The principal points in this exposition of their opinion, and which they keep constantly in view throughout the whole work, are, that the cineritious, grey, or gelatinous substance every where produces the nervous fibres, and that it is essentially the same in the ganglions, the spinal cord, the cerebellum, and the brain, though somewhat modified according to the different character and functions of the nerves proceeding from it.

We are unwilling to venture a hasty opinion on these leading parts of the authors' doctrines, as they are deserving much sober investigation, and appear to have been formed with much caution, and after much laboured research on their part. The propriety of using the term origin has been often called in question, as conveying false notions with regard to the connexions between different parts of the animal system. The manner in which it is employed in the present instance has been much objected to, and the notion of the nervous fibres being produced from any part has been ridiculed as unphilosophical. Now it is not at all certain that every part is really formed at the same time, so that it would be incorrect to speak of one part arising from, being produced, or perfected by, another. It is evident that the nervous system is essentially composed of two substances: one of a grey colour, in general pulpy and gelatinous; the other white, firmer, and formed of parallel fibres. It

is also evident that we always find a collection of the first, more or less in quantity, at the beginnings of the nervous filaments. We do not say that one part is the cause of another, but we cannot see any absurdity in supposing the nervous fibres to be produced in and dependent on the cineritious substance. We know that, to follow the accepted language of physiology, we must say that every part is produced immediately from the blood. Perhaps however, this will be found to be yet too sweeping a conclusion. The identity, or rather similarity, in structure and functions between the cineritious matter of the ganglions and of the other parts of the nervous system, is the other point which our authors think they have demonstrated; but which appears to have met with as much opposition as their opinions on the production of the nervous fibres. We are aware of some objections: the strength of the argument however is certainly at present with our authors. We wish we may not chance to meet again with physiological doctrines less ably supported by facts and by reasoning than the present.

In the next section we have a dissertation on the differences between animal and organic life, "*la vie automatique*." They point out some errors in the beautiful theory of Bichat; some of his opinions not being borne out sufficiently by facts. For instance, the position—that the organs of animal life are double and precisely alike in size form and situation, is not rigorously true. It is not correct also, that the organs of animal life only arrive at perfection by frequent exercise; and that those of organic life are perfect at the first moment they are called into action. The truth is, that organs exercise their functions exactly in proportion to their development: where the organs are complete at the instant of birth, their functions also are perfect, under whatever class we may choose to arrange them. The error has arisen from taking too partial a view of the functions of the animal system. We are, perhaps, all too fond of generalizing from limited observations on the human structure, without looking abroad through the rest of the animal creation.

Section 4. The part which immediately succeeds the nervous systems of the spine increases considerably in size as soon as it becomes contained within the cranium. It is to this part that the authors trace the "first visible origin of the greater

number of the nerves of the senses, of the fifth, sixth, and seventh pairs, as well as of the brain and cerebellum. It has in common with the spinal marrow, the anterior and posterior fissures, the commissure or transverse layer of union, the internal layer of cineritious substance, and the canals made visible by blowing." It is usually called the medulla oblongata, and its limits have been variously fixed by different authors. The nerves proceeding from it are commonly termed nerves of the brain, as it appears, very incorrectly. The authors call them the nerves of the head. They consider them as nervous systems wanting in the inferior orders of animals, and designed for the functions of the senses. In the sympathetic and spinal systems, the nerves are speedily detached from the mass: here, the nervous bands remain attached to the mass for a considerable space. We must be careful therefore not to confound the point of origin with the point of departure from the mass. As long as the band continues united to the mass they think that it continually receives some augmentation from the cineritious matter contained in it. Frequently the cineritious substance is collected into a body, a ganglion, in order still further to reinforce the nervous fibres. The arrangement according to the point of departure from the mass is certainly therefore defective, as this is often distant from their real origin.

Our authors reckon eleven pair of the nerves, in accordance with the opinion of the best anatomists. The origins of most of these have been followed by them to the medulla oblongata. The accuracy of their conclusion is not we think to be doubted. Their discoveries are earnests of what may yet be done by a careful examination of parts, the structure of which is supposed to be fully known. They bear honourable testimony to the talents and labour of our authors, and should dispose us to a fair confidence in their assertions where from want of knowledge or address we may not be able at once to prove them true. We can readily suppose that there are some who will consider all their pains as idle and unnecessary, who cannot see what advantage is ever to accrue from such minute investigations. It is certainly but too true that in general our knowledge of real structure is very inaccurate, and is wholly insuffi-

cient to guide us without error in examining the healthy or disordered functions of parts. But who is to say that we can never arrive nearer perfection, or that an intimate knowledge of organization is not the first step towards reaching it. The want of precise knowledge has ever been the greatest obstacle to improvement. For ourselves, and in respect to the present subject, we must say, that whoever has witnessed the careless routine in which the nervous system is shown in our anatomical schools, the exhibitions of melting brains and shrivelled preparations, could not be surprised if we were to remain for ever in the dark as to its structure and functions.

There can be no dispute about the origins of the accessory nerve, the hypoglossal nerve, the vocal nerve or nervus vagus, and the glosso-pharyngeal nerve. They bear a close resemblance in this respect to the nerves of the spine. "The following have this peculiarity, that the threads which are successively produced in the cineritious substance unite into one band before they quit the mass; which band is distinctly directed upwards, sometimes more, sometimes less visible, according as it is more or less covered by the neighbouring bands." It will not be within our limits to follow the authors through this part of their labours. The reader will find their opinions, expressed with sufficient correctness, in the report delivered to the Institute referred to in our last number. There is some slight difference, but not enough to affect the general result. "It is certain that all these nervous systems originate in cineritious substance, and by no means in what is called the medullary substance of the brain. It is also demonstrated that the different nervous systems are not perfect in the first instance, but are strengthened and perfected during their course. For this purpose, they are in some instances accompanied in their passage by the cineritious substance, increasing in size the further they are from their origin. In some cases the nerves in their passage cross a collection of cineritious substance, the latter producing additional nervous threads which unite to those already formed, and so on until the system is completed."

Section 5. On the difference of nerves. They consider the nerves as different throughout. There is a primitive difference in their internal structure, and an essential necessity therefore

for a difference of functions. They all originate in different masses of cineritious substance, and have often an apparent difference in structure. Our authors occupy themselves, we think rather idly, towards the end of the section, in refuting at length some marvellous opinions on the subject of animal magnetism, which it should seem are still in fashion in some parts of the continent.

In the next section they enter at great length on the functions of the external senses. The subject is handled with great ingenuity and exhibits many proofs of originality of thought and of accurate reasoning. One of their objects is to prove the perfection of each organ of sense individually considered, and the futility of the speculations of many eminent philosophers who argue for the necessity of an exertion of the judgment, and a combined action of different organs to make the sensation perfect. We look forward to no little instruction in seeing the same extensive spirit of research at work on the functions of the brain. Their remarks on the senses of sight, hearing, and touch are particulary interesting, and will correct many false notions which have crept into physiology from the philosophical writings of our most distinguished metaphysicians. This class of authors have certainly broached many incorrect, and some absurd opinions from attending too much to the sensations in man, and from an ignorance of many points in the comparative structure and functions of man and other living creatures. If all the physiological lectures of our authors were like these, we can readily account for their popularity.

They next proceed to the examination of the cerebellum and the brain. To avoid any confusion they consider the systems of organic life, those of voluntary motion, and of the senses, as entirely distinct; and only comprise under the term brain the nervous systems, which they suppose to be the special organs of the moral and intellectual qualities. It is placed in communication, and in reciprocal action and reaction with the other systems of the nerves. It no more arises from the nervous mass of the spine, than it gives origin to it. Like all the rest of the nervous systems it is composed of two substances, the grey and the white, or of cineritious matter and nervous fibres. The fibrous nature of the last

in the brain they consider as a point in structure abundantly demonstrated. The word medullary therefore, they wholly lay aside. In the common mode of exhibiting the organization of the brain by thin slices this appearance is scarcely visible, but by gentle scraping in the course of the fibres it is at all times *apparent*. Where from their superficial position the fibres are exposed to view without any preparation this structure is equally apparent. The cineritious matter is variously disposed, but its nature and offices are every where the same.

Most of the nerves of the senses have their first visible origin in the cineritious substance of the medulla oblongta, "*le grand renflement;*" it is to this part also that our authors trace the first visible origin of the cerebellum. These origins form on the outer side of the medulla a large band, the *corpus retiforme* or *processus cerebelli ad medullam*, which enlarges as it mounts, the fibres passing into the interior of the cerebellum. A short way after their entrance they meet with a mass of cineritious substance, the *corpus ciliare*, with which they become so intimately united as to render it impossible to follow their direction. This cineritious matter is the ganglion of the cerebellum and gives origin to many new fibres. The bands which pass out ramify into the various subdivisions of the cerebellum. One of the principal bands passes towards the median line, and contributes with the corresponding one of the opposite side to form the middle part of the cerebellum. This is a fundamental part found in all animals, whatever be the disposition of the brain in other respects. The lateral parts are superadded in different animals in various proportions. All these nervous fibres are covered at their peripheric extremities by cineritious substance. To exhibit these appearances nothing more is necessary than to make a vertical section in the direction of the primitive band so as to divide the ganglion into two equal parts.

So far they describe the nervous fibres diverging, and occupying a larger circumference. There is also another order of fibres which arise from the cineritious matter of the surface, and pass in the intervals of the fibres first described, which they cross, always converging towards the external and anterior margin, where they form a broad and thick fibrous

layer. The posterior and middle filaments pass transversely through the longitudinal bands of the medulla oblongata, which go to diverge in the hemispheres of the brain. The anterior filaments lie in front of these bands, in the form of a broad fibrous layer in man. They all unite in the middle line with their fellows of the opposite side, thereby joining the two hemispheres of the cerebellum, after the same manner as the hemispheres of the brain are united by the corpus callosum, &c. This part which now goes under the name of the pons varolii, or tuberculum annulare, is really then the great commissure of the cerebellum; and is always in a direct ratio to the size of the hemispheres, as the primitive bands are to that of the ganglion. The mammalia having smaller cerebella have also this commissure smaller. In man, where the cerebellum is so large and so fully developed, the commissure is particularly broad, covering the origins of many of the nerves which are exposed in other animals. This commissure is not found, or there is not, to use the common language, any pons in fishes, reptiles, and birds: for these animals have no lateral parts to their cerebellum, and consequently no converging fibres to form it. But all animals have a commissure of what our authors call the fundamental part, independent of the commissure of the lateral parts, or pons. This commissure is formed by the soft and fine fibrous layers from the superior and inferior parts of the middle or fundamental portion, commonly called the superior and inferior valves.

The cerebellum does not contain more cineritious substance in proportion to its size than the brain. The mistake arises from our generally making horizontal sections of the last, and vertical ones of the cerebellum; reverse them and the brain will appear to contain the most cineritious matter. The cerebellum is not always in proportion to the brain, as to size or development: it is not so large proportionally to the brain, in children as in adults.

In describing the brain, our authors confine themselves principally to the human subject, as the one in which the organization of the brain is the most complete, and of which the different parts can be represented in the most sensible manner:

"The brain consisting of many divisions, the functions of

which are totally different, there are found many primitive bands which by their development contribute to form it. All these bands are composed gradually by fibres produced in the cineritious substance of the medulla oblongata (*du grande renflement.*) They are to be considered as the first visible rudiments, or commencement of the brain, having communication and reciprocal actions with the surrounding systems. Among these primitive bands we place the anterior and posterior pyramidal eminences, the bands which proceed from the corpora olivaria, the longitudinal nervous bands which contribute to form the fourth ventricle, and some others which are concealed in the body of the medulla oblongata."

With but one exception the bands pass into the hemispheres, keeping the same side in which they originate. With regard to the anterior pyramidal eminences the case is reversed. Here the fibres which arise on the right side unite in the first instance into a few cords which pass obliquely over to the left. In the same manner they pass from left to right, each cord passing over one and under another of the opposite side forming a sort of interweaving like basket-work. This decussation occupies a space of four or five lines at the lower extremity of the medulla oblongata. The bands afterwards pass up on the anterior surface of the medulla, gradually acquiring the pyramidal form. This appearance is very easily demonstrated, and is totally distinct from the transverse layer or commissure, visible both above and below this part in the anterior fissure of the medulla. The authors hint that as the fibres of the anterior pyramids are the only ones which decussate, so it is only an injury of that part of the brain which is a continuation or development of these bodies, which affects in its consequences the opposite side of the body. Injuries of the cerebellum, of the posterior lobe, and of part of the middle convolutions of the brain, would affect the same side, these parts being continuations of fibres which do not cross. This is an interesting point and deserving of further research.

The pyramids as soon as they enter the tuberculum annulare divide into many bands, which lie in a considerable mass of cineritious substance: from this many new fibres arise and join them. It is regarded as a true ganglion. The bands are

so much enlarged when they pass out above as to form nearly two thirds of the crura cerebri, "*les grands fasceaux fibreux.*" The transverse nervous bands seen in the tuberculum annulare, are, as before mentioned, the commissures of the cerebellum. This structure may be seen by making a vertical incision curved in the direction from the pyramids to the crura cerebri. The great fibrous bands contain in their interior a considerable quantity of cineritious matter, from which they are continually reinforced by new fibres. The nervous fibres are continued upwards, and gradually terminate in forming a nervous layer, covered by cineritious matter externally. "Thus the pyramidal bands may be traced from their point of origin until they are perfected and terminate in the inferior, anterior, and external convolutions of the front and middle lobes. These parts of the brain are always in proportion to the pyramids, the cineritious matter of the tuberculum annulare, and the crura cerebri in other animals, as well as in man." The same principle of structure is followed in the formation of the posterior lobe, and of the convolutions situated at the upper and internal edge of each hemisphere. The corpora olivaria they consider to be ganglions. From each there ascends a large band, joined with the posterior bands from the medulla oblongata. They cross the tuberculum annulare and form the posterior and inner third of each crus. They receive but little addition until they enter a thick mass of cineritious matter, known generally by the name of thalamus opticus. In this ganglion is to be seen a large quantity of nervous filaments, which ascend, and at their exit upwards unite into bands, which immediately diverge in a radiating form and traverse another large mass of cineritious substance, the corpora striata. In this ganglion they are again reinforced and eventually form the posterior convolutions and those placed internally at the upper part of each hemisphere.

The canals mentioned as existing in the nervous mass of the spine are prolonged between the two ascending divisions of nervous fibres just described, following the course of each crus. At the part where the fibres pass out from the thalami there is so firm a union between these and the transverse con-

verging fibres which go to form the commissures, as to render all further separation impossible without laceration.

The thalami optici and corpora striata are in this view considered as true ganglions, in which the nervous fibres are increased in number and size, and are prepared by degrees to form their respective convolutions. The latter are regarded as the perfect organs, and the preceding as the preparatory apparatus. " In examining under a general point of view the manner in which nature proceeds to the primary formation of the two hemispheres of the brain, we see that the original bands are produced in the cineritious substance of the medulla oblongata, and reinforced in different places by particular masses of cineritious substance; that consequently all follow the same laws as are observed in the nervous systems of the abdomen and the thorax, of the vertebral column, of the nerves of the head, and of the cerebellum."

All the parts of one side of the brain communicate with corresponding ones of the opposite side by transverse, or as they generally call them, by converging nervous fibres which unite at the median line. The ascending or diverging fibres have been followed into the cortical or cineritious substance, which they penetrate to some depth, making the internal part of a lighter grey than the external. How they finally terminate is not positively stated. It is thought probable that new nervous fibres are produced in the cineritious matter intimately connected with those whose course we have been following. The existence of the converging fibres may be more readily demonstrated than even that of the diverging. They may be seen at the bottom of every convolution, decussating the latter, with which they are intermingled. As they advance inwards they form nervous bands and layers which go out of one hemisphere to meet corresponding ones from the other, forming the different junctions or commissures. The principal converging bands are the corpus callosum, the fornix and lyra, and the anterior and posterior commissures. The size of the different bands of reunion is always in proportion to that of the parts they unite in the different classes of animals.

" The brain is then formed, and perfected in the same

manner as the other nervous systems. The analogous parts of the two hemispheres have, as the analogous nervous systems of each side of the body, their reunions in the median line. Who in the conformity of these laws can mistake the character of truth? Can any one still prefer the arbitrary, confused, and incoherent method of studying the brain to our method, so simple, sure, and so well followed up?"

The authors proceed to describe the different cavities of the brain, and the structure of the convolutions. The diverging fibres, after decussating the converging fibres at the outer side of the great ventricles, which they thus effectually close, divide from each other more and more, and terminate by forming a fibrous expansion. They are unequal in length; some ending immediately beyond the outer sides of the ventricles, others continuing further; the interior having the longest course. The nervous fibres are thus lost successively in the cineritious substance, the middle ones only being continued to the extremity, and forming two layers touching each other in the middle of each convolution, slightly agglutinated by a mucous or fine cellular tissue. They are covered throughout by a layer of cineritious substance of nearly uniform thickness. The same takes place in the cerebellum, but the divisions are more minute. Each convolution then is composed of converging and diverging fibres, and of a cineritious envelop. From the structure just described arises the possibility of separating the two layers without injuring either. The division in each convolution is easily demonstrated by blowing gently, or directing a small stream of water on the middle line. The fact is rendered still more evident if the part be hardened in nitrous or muriatic acid diluted with alcohol, or if it be boiled in oil. If the part at which the different fibres decussate at the outer side of the ventricles is ruptured, the whole of each hemisphere may be unfolded as it were, and made to appear as a soft bag, white and fibrous on the internal, and grey on the exterior surface. The pineal and pituitary glands are considered to be ganglions, as are also the corpora albicantia. Our authors treat also of many minor points of structure in different parts of the brain which we must wholly omit notic-

ing, hoping that enough has been given to convey to the reader some notion of their principal opinions.

In this imperfect sketch we have passed over the reasons given by Gall and Spurzheim in support of their physiological opinions, many of which are very forcible: we have also omitted the answers given to different objections made to several parts of their doctrine. If we have not expressed any opinion as to the merits of the physiological doctrines introduced throughout this work, it is because we do not think ourselves qualified at once to decide on them, and because they rest on facts the existence of which may be ascertained by any of our readers. The anatomical details will be found strictly correct. From a want of rightly comprehending the authors' meaning, or of sufficient address, it may happen that a superficial examiner may be disappointed in his search. The fault however will be with him. There is never any ambiguity of expression, or any confusion in the descriptions; they are all lucid, and rather diffuse. The latter circumstance we think an advantage in describing structure which is delicate, and on which there has been so much misunderstanding. Conciseness is often overrated, and more than counterbalanced by the want of perspicuity which frequently attends it.

Whatever be the ultimate fate of their physiological opinions, anatomy must always gratefully acknowledge the labours of our authors. The great mass of anatomical information collected in this volume makes it highly valuable. They have we think taken the true route, and one unknown to, or imperfectly seen by any of their predecessors. They have made considerable advances; but the termination is yet far in the distance, and it will require long continued and extensive research before we can hope to reach it. Whilst anatomy possesses such intelligent and indefatigable labourers as Gall and Spurzheim, and men endowed with such ample talents as Sœmerring and Cuvier, we may yet expect great things.

The subject is one of the highest interest; and the prosecution of inquiries into the organization, functions, and diseases of the brain and nervous system, is the more to be encouraged as it is notorious that the sum of our present knowledge is

trifling indeed in amount, when compared with what is yet wanting. We cannot help lamenting the apathy of many who neglect, and the absurdity of others who ridicule such labours as the present. Few and of little import are the additions made of late years to physiological knowledge by the anatomists of this country. The appearance will be still more meagre if contrasted with the rich discoveries made by some of the ardent anatomists of the continent. It would be out of place to enter here on the causes of this deficiency on our part: the two principal ones are the total neglect of the higher parts of physiology in our anatomical schools, and the almost exclusive encouragement given to surgical anatomy. We may well prize our chemical, but we have no cause to boast at present of our anatomical philosophers.

The plates are without exception the most beautiful and the truest anatomical pictures of the brain which we have ever seen. The magnificent plates of Vicq d'Azyr appear hard in the comparison, and they are in many minute points confessedly inaccurate; and from their being composed from views chiefly of horizontal sections manifestly insufficient. The beautiful plate of the basis of the brain by Sœmerring, the best we possessed and by the most perfect anatomist of the present day, is not we think equal to a corresponding plate in the present work. They have another great merit also besides that of being well executed,—that of being well contrived to exhibit the structure of the parts they represent.

ORIGINAL PAPERS.

On the separation of the colouring matter of the Coccus Cacti *or Cochineal of a* Lilac *colour, by the decomposition of acetite of lead and sulphate of zinc. By* James Cutbush, *Professor of Chemistry, Mineralogy, and Natural Philosophy in St. John's College.*

The four following experiments and observations were drawn from a phenomenon, which accidently occurred on mixing acetite of lead and sulphate of zinc with cochineal. The peculiar colour which was produced struck my attention the more forcibly, as it is well known that several of the metallic salts have the property of separating the colouring matter of a red, and more frequently of a crimson colour. I found, that when acetite of lead and cochineal were mixed, no phenomenon ensued; but that when sulphate of zinc was added, an immediate decomposition was the consequence; and the colouring matter of the cochineal was precipitated, not of a red, but of a lilac colour. I was anxious, therefore, to determine whether the effect ensued by the agency of the acetite of zinc, or by that of the sulphate of lead, which were formed in the process. To ascertain this circumstance, the following experiments were instituted.

Experiment 1. Acetite of lead triturated with cochineal and water, afforded a purple colour.

Exp. 2. Sulphate of zinc mixed in a similar manner, gave a dull red colour.

Exp. 3. Acetite of zinc produced no material alteration. The effect however, was more like that of the solution of tin.

Exp. 4. Cochineal triturated with sulphate of zinc and acetite of lead, produced a lilac coloured precipitate with water.

These few experiments at once determine, that neither the acetite of lead, the sulphate nor the acetite of zinc had any agency in changing the colour of the *colouring matter;* but, that the sulphate of lead, at the moment of its formation, by the double decomposition which ensued, had the effect of

altering it so as to destroy its original colour, and of precipitating it of a lilac hue. On drying the precipitate, the colour appeared uniform; and so far as I may judge, seems to be a chemical compound of the sulphate and colouring matter. From the presence of the sulphate, as a body or base, and from the uniformity of the colour, I think it probable that it might be employed advantageously as a pigment for miniature painting; but, with respect to its durability, I am not yet determined.

As to the property which certain earthy and metallic salts possess, of precipitating the colouring matter of cochineal, for the operations of the dyer, it is well known that the chemical experiments which have been instituted on the subject, have led to better processes, and more correct and more desirable results. But, as regards the changes which take place in certain processes, not only in the substances themselves, but in the rays of light, in order to produce colour it is necessary to obtain a knowledge of all the circumstances concerned, to take into consideration the affinities or relations which such substances, or compounds so formed, have to their constituent parts, as well as to the rays of light. Bancroft, Berthollet, and more lately, Dr. Conover (in the last volume of the American Philosophical Transactions,) have considered this subject with becoming attention, in which they have paid every respect to the doctrine and opinions of sir Isaac Newton.

It is not our intention, however, to enlarge on this subject, nor to notice the theories and opinions which have been formed; but only to add, that an acquaintance with such branches of optics as relate to the laws and properties of light, with its connexion to chemical subjects, will lead to more correct deductions on the subject of colours. Dr. Conover, in a very able and ingenious essay already quoted, has shown the importance of such knowledge as applied to the arts; and has treated the subject, with its connexion to the arts in so clear a light, that but little remains to put his principles into practice. I allude to the formation and fixing of certain colours on porcelain, &c. by the agency of certain *determinate* temperatures. In the phenomenon before noticed, the production of the lilac colour, and indeed the violet and purple colours, will be found

to be compounds of red and blue in different proportions, so that a knowledge of the laws and properties of light must eventually lead to correct deductions, and show how far certain rays are concerned. These opinions have been ably supported by Mr. Dalton, in an essay on the prismatic colours. Considering the lilac a compound colour, might we not obtain a *blue* pigment by using either less of the cochineal, or a larger quantity of the sulphate of zinc and the acetite of lead, by which the red colour might be altogether discharged? This experiment I intend to perform at a subsequent period.

The further we carry our investigations, the more we perceive it necessary, in order to come at the truth, to consider the primary causes which operate in effecting the different changes; and, of course, the union of some, and the extrication of other principles, or constituent parts, to the accomplishment of different processes. These facts are the more interesting where a number of substances are concerned; where complicated affinities take place; and where the results are governed, for the greater part, by the interposing medium of light and caloric. If the production of colour be mechanical, which is contrary to fact, such changes in colour must have no relation with chemical changes; but, as chemical changes, in a variety of cases, are followed by a corresponding change in the rays of light (more especially in dying) we may establish it as a *datum*, that chemical results are affected by *all* the circumstances which can in any manner influence them. Colours it is true are formed in the solar light; the various tinges of colour resulting from the absorption of some of the rays of light, and the reflection of others. By the art of dying, therefore, a substance possessing the property of reflecting particular coloured rays, according to Bancroft, is transferred to the surface of another body. The preparation of the stuff, previously to receiving the colour, the menstruum or vehicle through which the colouring matter is transferred, and the different affinities which take place in these processes, show at once the truth of what we have advanced. But as this is irrelevant to our present subject, we shall not enlarge upon it.

INTELLIGENCE,

Medical and Philosophical.

On the Structure of the Eyes both of Man and Biras.

A PAPER by Mr. Champion was read before the Royal Society, on the structure of the eyes both of man and birds, particularly in relation to that faculty of the eye which enables it to adjust its focus to the distance of the object. The author examined the different conjectures and theories which have been proposed to account for the circumstance, and explain how the eye can have perfect images of objects at very different distances. It has been generally agreed, that the eye must have some contractile power; but the existence of any organ capable of such a function has never been ascertained. Mr. C. in examining the eyes of an eagle, was led to discover the existence of a small muscle attached to the sclerotica and capable of contracting the eye, in a manner equal to effect the necessary change in the focal distance. The same organ he discovered in some other birds, and hence he inferred that something analogous exists in the human eye. He observed, that images pass before the eyes of maniacs as vividly and distinctly without any sensible objects, as they do over those of some persons from objects within the focal distance of their eyes.

Experiments on Light.

A paper by Count Rumford was also read, on the nature of light. The count, firmly persuaded, that it is of great commercial importance to increase the quantity of light for men's use, and that this luminous *nothing* has no more distinct existence than sound, proceeded to make a number of little experiments on wax tapers and a photometer. He began by weighing the combustible matter consumed or transformed during a given time, and compared the quantity of light emitted in proportion to the wax burnt. In nine such experiments he satisfied himself that the light emitted bore no invariable pro-

portion to the quantity of combustible matter consumed, but that, with small tapers, which yielded very little light, there was a very considerable increase of heat. Here he was induced to make some observations on the nature of heat, or heated bodies, so far as their luminous qualities were concerned; all of which, he thought, tended to confirm his opinion, that there is no such matter in existence as light, and that philosophers may for ever torture their imaginations about its nature and qualities, without ever being a tittle the wiser. He observed, that no person ever looked for the nature and properties of sound in fulminating powder, and it is equally idle to look for those of light in combustible bodies. He has however a very philanthropic and important discovery to make, namely, a new invention of his *own*, a polyflame lamp, consisting of a number of burners, with wicks flat like a ribbon, and so placed one at the side of another that the air can pass between them, at the same time that they are duly supplied with oil. The flat wicks arranged in this manner, side by side, supplied with oil, and covered with a large glass which rose several inches above the flame, yielded as much light as forty candles! The count concludes with expressing his own liberality in thus publishing his discovery, and declares himself ready to give every possible information in his power to any person who may wish to construct such lamps; but modestly adds, that his own is not made quite so well as he could wish, and that his apparatus is still capable of further improvement.

January 23, 1812.

The Jacksonian Prize of the Royal College of Surgeons in London, for the year 1811, has been adjudged to Mr. Joseph Hodgson, of Bucklesbury, London, for his dissertation on the diseases of Arteries and Veins; comprising the pathology and treatment of Aneurism and wounded Arteries.

Professor Leslie's Discovery.

The experiments of professor Leslie, to produce ice by evaporation in the air pump, have been varied and extended in France by Messrs. Clement and Desormes: they have pro-

posed to apply the evaporation, in vacuo, on a large scale, to the drying of gunpowder; which, being done without fire, will be attended with no danger.

The French chemists are engaged in endeavouring to apply the evaporation in vacuo (before stated) to the drying and preserving fruit and vegetables. It may be easily conceived of what advantage this process may be, particularly in the army and navy, by preserving, unchanged, alimentary substances, and also by diminishing their weight and bulk, when they are to be sent to distant parts of the world.

In consequence of this paragraph, we observe that Mr. Leslie has addressed a letter to the editor of the Caledonian Mercury, and we can bear testimony to the truth of the professor's claims to the discovery; for we personally witnessed the demonstration of his theory while in London last year, when he freely explained the various important uses to which the principle might be applied, particularly by the introduction of the apparatus into hospitals in sultry climates, and for its general utility in domestic purposes. We understand that he has, since his return to Edinburgh, improved his coolers, so as to produce ice in large quantities, by powers much inferior to those he first used.

PENNSYLVANIA HOSPITAL.

AT a meeting of the Contributors to the Pennsylvania Hospital, held on the 4th of May, 1812, the following Contributors were elected officers of that institution, for the ensuing year.

MANAGERS.

Samuel Coates,	Edward Pennington,
Pattison Hartshorne,	Samuel W. Fisher,
Joseph Lownes,	Thomas P. Cope,
Joseph S. Morris,	Zaccheus Collins,
Thomas Stewardson,	Reeve Lewis,
Lawrence Seckel,	Joseph Watson.

TREASURER.

Joseph S. Lewis.

At a meeting of the Managers, held on the 11th of May, 1812, the following Contributors were elected Physicians of the House, viz.

Thomas Parke,
Benjamin Rush,
Philip Syng Physick,
Benjamin Smith Barton,
John Syng Dorsey,
Joseph Hartshorne,
Thomas C. James,
for the Lying-in Department.
Samuel Calhoun, for the out Patients.

At a meeting of the President and Directors of the Pennsylvania Academy of the Fine Arts, May 11th, 1812,

Nathaniel Chapman, M. D. was elected Professor of Anatomy in the said Institution.

WM. SMITH, Sec'ry of the Academy.

At a meeting of the Philadelphia Medical Society, the following Members were elected Officers for the ensuing year.

PRESIDENT.

Benjamin S. Barton, M. D.

VICE-PRESIDENTS.

Philip S. Physick, M. D.
N. Chapman, M. D.

SECRETARIES.

Joseph Hartshorne, M. D.
A. Benezet, M. D.

ORATOR.

Charles Caldwell, M. D.

METEOROLOGICAL OBSERVATIONS.

State of the weather, &c. at Philadelphia during the first six months of 1812.

JANUARY.

Thermometer—Lowest, at 8 A. M. 6. 22d day of the month.
Highest, 3 P. M. 41. 31st.
Mean . . . 28.

Winds—Chiefly west and northwest.

January has been a real winter month throughout—the 22d day of the month was colder than it has been since December 23d, 1796—but one snow of any consequence on the 18th, when the river was frozen—not much rain this month.

FEBRUARY.

Thermometer—Lowest, at 8 A. M. 14. 12th day of the month.
Highest, 3 P. M. 45. 8th.
Mean . . . 30.

Winds—Very changeable—mostly northerly.

February has appeared in its usual form, changeable and unpleasant—three or four snows one or two inches deep—not much rain. The ice in the river broke up on the third. The winter continued severe to the end of the month. Frequent earthquakes to the southward and westward.

The measles which were observed last autumn, now more prevalent—the first case recorded on the books of the Philadelphia Dispensary, October 19, 1811, in the northwest part of the city—the disease is mild with the usual symptoms. Whooping cough also prevalent.

MARCH.

Thermometer—Lowest, at 8 A. M. 22. 2d day of the month.
Highest, 3 P. M. 60. 27th.
Mean . . . 38.

Winds—Very variable from all points of the compass.

March has been a cold and very unpleasant month, promising a backward spring—little rain.

On the 26th a dreadful earthquake felt on the Spanish Main, by which the cities of Caraccas, Laguira and several others were destroyed.

April.

Thermometer—Lowest, at 8 A. M. 32. 12th day of the month.
Highest, 3 P. M. 75. 19th.
Mean . . . 45.

Winds—Much easterly.

April, on the whole, has been a cold and dry month, unfavourable to vegetation. The 19th was warmer than any day in April that is remembered, although the season was in general cold.

May.

Thermometer—Lowest, at 8 A. M. 42. 5th day of the month.
Highest, 3 P. M. 75. 31st.
Mean . . . 50.

Winds—Chiefly eastwardly.

Snow on the 4th, and ice on the 5th. More frequent rains—cold season and backward vegetation.

The newspapers give an account of a considerable volcanic eruption of Mount Souffriere in the island of St. Vincents on the first day of this month.

June.

Thermometer—Lowest, at 8 A. M. 61. 5th day of the month.
Highest, 3 P. M. 82. 8th.
Mean . . . 70.

Winds—Northerly and westerly.

Heavy rains and thunder on the 8th. Heavy rains on the 16th and 24th, also on the 27th with very high wind and loud thunder—good prospects of harvest—the weather unusually cool.

The measles continued to the end of this month—little or no small-pox in the city for the last six months—vaccination generally practised.

RECENT FOREIGN PUBLICATIONS.

Dissertation on the Bite of a Rabid Animal. By J. Gilman.

Practical Observations on Debilities. By J. Caton.

A View of the Nervous Temperament, being a practical treatise on nervous, bilious, stomach, and liver complaints, &c. By T. Trotter, M. D. Third edition, with large additions.

Observations on the Diseases of the Liver. By T. Mills, M. D.

A Treatise on the Management of Infants, &c. By J. Syer, Esq. Surgeon.

Essay on Scrofula. By J. Armstrong.

The Surgical Anatomy of the Head and Neck. By A. Burns.

On the Operation of largely puncturing the Capsule of the Chrystaline Humor, in order to promote the Absorption of the Cataract. By J. Ware, F. R. S.

A Treatise on some practical points relating to the Diseases of the Eye. By the late John C. Saunders.

The Principles of Physiological and Physical Science, &c. By Richard Saumarez, Esq. Surgeon.

Observations on the Contracted Intestinum Rectum and Mode of Treatment. By W. White.

Review of Mr. E. Home's Practical Observations on the Diseases of the Prostate Gland. By Jesse Foot.

An Examination of the Mineralized Remains of Vegetables and Animals of the Antediluvian World, &c. By J. Parkinson. Vol. 3.

General Zoology, &c. By George Shaw, M. D. F. R. S. Vol. 8. being the 2d vol. of Birds.

Essays on Natural History and Rural Economy. By T. Walker.

PROPOSED FOREIGN PUBLICATIONS.

Dr. Cheyne of Dublin, has in the press, a curious and extensive work on Apoplexy, with plates illustrative of the disease.

Sir Humphrey Davy has in the press, a volume of the Elements of Chemistry.

Dr. Stokes of Chesterfield, has just finished, in 4 vols. his Botanica Medica.

Mr. Adams has in the press, a Treatise on the Morbid Affections of the Eye and its Appendages.

Dr. Sutton is preparing for publication, Observations on the injurious Effects of Mercury in various diseases.

Dr. Lipscombe has nearly ready for the press, a Grammar of the Elements of Medicine.

Dr. Delys of Birmingham, has in the press, a translation of Richerand's Elements of Physiology, from the 5th and last edition illustrated by notes, and accompanied with a comparative view of the state of physiology in England and on the continent.

Professor John Leslie of Edinburgh, has in the press, a View of the Facts ascertained concerning Heat, and its relations with Air and Moisture.

RECENT AMERICAN PUBLICATIONS.

An Account of the New York Hospital. New York, printed by Collins & Co. 1811. 8vo. To which is appended, a Catalogue of Books belonging to the New York Hospital Library.

An Essay on the Organic Diseases and Lesions of the Heart and Great Vessels. From the clinical lectures of J. N. Corvisart, first physician of their imperial and royal majesties; officer of the school of medicine of Paris, and of the imperial college of France; physician in chief of the hospital of La Charité, &c. &c. published, under his inspection, by C. E. Horeau, doctor in medicine, surgeon of the infirmary and house of the emperor and king. Translated from the French, with notes, by Jacob Gates, M. M. S. S. Philadelphia, Anthony Finley.

A Dissertation on the Use and Abuse of Tobacco, wherein the advantages and disadvantages attending the consumption of that entertaining weed, are particularly considered. Humbly addressed to all Tobacco Consumers, but especially those among Religious People. Second American edition. By Adam Clarke, L. L. D. Newburyport, Thomas & Whipple.

Original Communications for this work will be thankfully received by Thomas Dobson, No. 41, south Second-street.

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